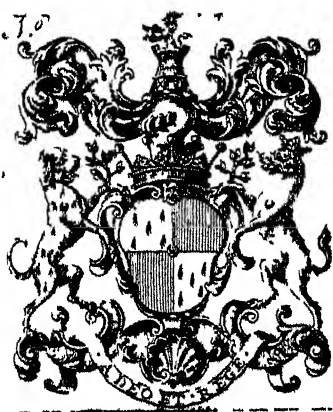


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AGRICULTURAL RESEARCH INSTITUTE
PUSA

PHILOSOPHICAL
TRANSACTIONS,
GIVING SOME
ACCOUNT

OF THE
Present Undertakings, Studies, *and* Labours,
OF THE
INGENIOUS,
IN MANY
Confiderable Parts of the WORLD.

V.O L. LIV. For the Year 1764.

L O N D O N :

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THE Committee appointed by the *Royal Society* to direct the publication of the *Philosophical Transactions*, take this opportunity to acquaint the public, that it fully appears, as well from the council-books and journals of the Society, as from repeated declarations, which have been made in several former *Transactions*, that the printing of them was always, from time to time, the single act of the respective Secretaries, till the Forty-seventh Volume. And this information was thought the more necessary, not only as it has been the common opinion, that they were published by the authority, and under the direction, of the Society itself; but also, because several authors, both at home and abroad, have in their writings called them the *Transactions of the Royal Society*. Whereas in truth the Society, as a body, never did interest themselves any further in their publication, than by occasionally recommending the revival of them to some of their secretaries, when, from the particular circumstances of their affairs, the *Transactions* had happened for any length of time to be intermitted. And this seems principally to have been done with a view to satisfy the public, that their usual meetings were then continued for the improvement of knowledge, and benefit of mankind, the great ends of their first institution by the Royal Charters, and which they have ever since steadily pursued.

But the Society being of late years greatly enlarged, and their communications more numerous, it was thought advisable, that a Committee of their Members should be appointed to reconsider the papers read before them, and select out of them such, as they

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should judge most proper for publication in the future *Transactions*; which was accordingly done upon the 26th of March 1752. And the grounds of their choice are, and will continue to be, the importance or singularity of the subjects, or the advantageous manner of treating them; without pretending to answer for the certainty of the facts, or propriety of the reasonings, contained in the several papers so published, which must still rest on the credit or judgment of their respective authors.

It is likewise necessary on this occasion to remark, that it is an established rule of the Society, to which they will always adhere, never to give their opinion, as a body, upon any subject, either of nature or art, that comes before them. And therefore the thanks, which are frequently proposed from the chair, to be given to the authors of such papers, as are read at their accustomed meetings, or to the persons, through whose hands they receive them, are to be considered in no other light, than as a matter of civility, in return for the respect shewn to the Society by those communications. The like also is to be said with regard to the several projects, inventions, and curiosities of various kinds, which are often exhibited to the Society; the authors whereof, or those who exhibit them, frequently take the liberty to report, and even to certify in the public news-papers, that they have met with the highest applause and approbation. And therefore it is hoped, that no regard will hereafter be paid to such reports, and public notices; which in some instances have been too lightly credited, to the dishonour of the Society.

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PHILOSOPHICAL TRANSACTIONS.

1. *An Account of a Mummy, inspected at London 1763. In a Letter to William Heberden, M. D. F. R. S. from John Hadley, M. D. F. R. S.*

Dear Sir ;

Read Jan. 12, 1764. **O**N the 16th of December 1763, Dr. Wollaston, Dr. Blanshard, Dr. Hunter, Dr. Petit, the Rev. Mr. Egerton Leigh, and Mr. Hunter, met at my house; that we might together inspect a Mummy, which I had received from the Museum of the Royal Society.

Our intention was; to examine the manner, in which this piece of antiquity had been put together; to compare it with the accounts given of these preparations by ancient authors; and to see, whether there were any traces left of the softer parts; and, if so, by what means they had been preserved. A greater

number of authors have written on this subject, than I was aware of; so that, in all probability, we have not made any new discoveries. I enclose to you the result of our enquiry; and the few obvious reflexions which occurred. If they appear to contain any thing worthy notice, you will please to communicate them to the Royal Society. I am,

Dear Sir,

Your most obedient and

Charter-House,
Jan. 10th, 1764.

Obliged humble Servant,

John Hadley.

THE Mummy, which is the subject of the following pages, is the first article in Dr. Grew's Catalogue of the rarities of the Royal Society. He informs us, that it was a present from Henry Duke of Norfolk; and was an entire one, taken out of the Royal Pyramids. He then proceeds to describe the manner, in which the several parts were wrapped up; but this he has not done exactly: as most of these very parts had evidently never been opened, till we examined them: and were then found in a very different state from that in which they are represented by him.

This Mummy had been greatly injured, before it came into our hands; the head had been taken off from the body; and the wrappers, with which they had been united, having been destroyed, the cavity of the thorax was found open towards the neck: and part of the upper crust, with the clavicles, having been also broken away, the heads of the *ossa humeri* presented themselves covered with a thin coat of pitch.

The feet also had been broken off from the legs; and were fixed, by wires, to the end of the wooden case in which the Mummy lay.

The outward painted covering, which reached from the upper part of the chest nearly to the bottom of the legs; had been removed and fastened on again by a great number of ordinary nails, driven up to the head into the substance of the Mummy. This had most probably been done by those, who had orders some years since to repair it; and by this, and by the manner in which they had fastened on the feet, they seem to have done their work in a most clumsy manner.

This whole external covering of the fore part of the Mummy consisted of several folds of broad pieces of linnen cloth ; made to adhere together by some viscous matter, which had not yet lost its property : and the whole had received an additional degree of strength and substance from the coat of paint laid on. The figures, which were not entirely defaced, were so much of the same kind with those which the writers on this subject have described, as to make any account of them here needless : and, indeed, they were all so much injured, as to render a particular description of them very difficult, if not impossible.

There were not the left remains of hair or integuments, on any part of the head ; some parts of the skull were quite bare ; particularly about the temporal bones : which had the natural polish, and appeared in every respect like the bones of an ordinary skull. To other parts of the skull adhered several folds of pitched linnen ; which together were near half an inch in thickness : on removing them, they were found to have been in actual contact with the bone ; so that the integuments must have been taken away, before the wrappers were at first applied.

The under jaw was lost : and the superior maxillary, sphenoidal and ethmoidal bones were broken away ; the *foramen occipitale* was stopped up with pitch, with which also the inner part of the skull was lined ; this seemed to have been poured in at the *foramen*, and made to apply to the several parts of the inside of the skull, by turning the head in different directions ; the wave of the melted pitch from such motion appearing very plain. The inside of the skull was in many places covered very thinly ; and, in some few, which
the

the fluid pitch had missed, it was quite bare. The pitch, which stopped up the *foramen occipitale*, had on it the impresson of one of the *vertebræ* of the neck; and externally about the *foramen* adhered a considerable quantity of pitch.

The outward painted covering being removed, nothing but linnen fillets were to be seen: which enclosed the whole Mummy.

These fillets were of different breadths; the greater part about an inch and a half, those about the feet much broader: they were torn longitudinally; those few that had a selvage having it on one side only; the uppermost fillets were of a degree of fineness nearly equal to what is now sold in the shops for 2s. 4d. *per* yard, under the name of long lawn; and were woven something after the manner of Russia-sheeting: the fillets were of a brown colour, and in some measure rotten. These outward fillets seemed to owe their colour to having been steeped in some gummy solution; as the inner ones were in pitch.

The fillets immediately under the painted covering lay in a transverse direction; under these, which were many double, they lay oblique, diagonally from the shoulders to the *ilia*. Under these the fillets were broader, some nearly three inches; and lay longitudinally from the neck to the feet, and also from the shoulders down the sides; on which there was a remarkable thickness of these longitudinal fillets: under these they were again transverse, and under these again oblique.

The fillets in general externally did not adhere to each other; but, though pieces of a considerable length could be taken off intire, yet (from the great age)

so tender was the texture of the cloth, that it was impossible regularly to unroll them.

As the outward fillets were removed, those that next presented themselves had been evidently steeped in pitch, and were in general coarser, in folds, and more irregularly laid on; as they were more distant from the surface. The inner filleting of all was so impregnated with pitch, as to form with it one hard black brittle mass; and had been burned nearly to a coal. On breaking this, it appeared in many places as if filled with a white efflorescence: like that observable on the outside of *pyrites*, which have been exposed to the air. This efflorescence however had nothing saline to the taste; and did not dissolve in water: but instantly disappeared, on bringing it near enough to the fire to be slightly heated; and was soluble in spirit of wine.

In the cavity of the *abdomen* we found several small pieces of bone, which had the appearance of dry oak, mixed with crumbled pitch; under this was found more solid pitch, which adhered to the spine.

After cutting away the mass of cloth and pitch which covered the *thorax*; we found, the arms had been laid straight down by the sides of the chest, and the ulna and radius bent upwards, and laid with the hands across upon the breast, the right hand being uppermost.

The bones of the fingers were lost; but the metacarpal bones were found, broken off, and fallen into the thorax.

The filleting, which went round the upper part of the body, included the arms also; but they had evidently

dently been first wrapped separately, then laid up in the position in which we found them, and the hollows which they formed filled up with pieces of pitched cloth.

In the cavity of the thorax there was also a considerable quantity of crumbled pitch and splinters of dry bone; and, as in the progress of this examination we continually found, that some of the bones did, as we laid them bare, separate into such splinters; it is very probable, that this appearance was owing to the Mummy's having been handled in a rough manner, and much shaken by the persons who had driven it full of nails, when they were employed to repair the outside of it.

On our first opening a way into the *thorax*, we imagined the ribs were destroyed; but, upon a more accurate examination, they were found entire; but so bedded in the pitch, and so black, and burned into the mass, as to make it difficult to distinguish these very different substances from each other.

The bones of the spine and of the *pelvis* were in the same state with the ribs; only rather more burned.

There was a considerable thickness of hard solid pitch lining the cavity of the *thorax*; this had been evidently liquified, and poured in; and retained that glossy appearance on its surface, which is observable on pitch that is suffered to cool without being disturbed.

On breaking through this hard crust of pitch, to examine the *vertebræ* and the ribs, the pitch, which was under this crust and nearest to the bones, was crumbly and soft; and, on being exposed to the air, grew perfectly moist, in a very short time.

The lower extremities were wrapped separately in fillets to nearly their natural size, and then bound together; the interstices being rammed full of pitched rags.

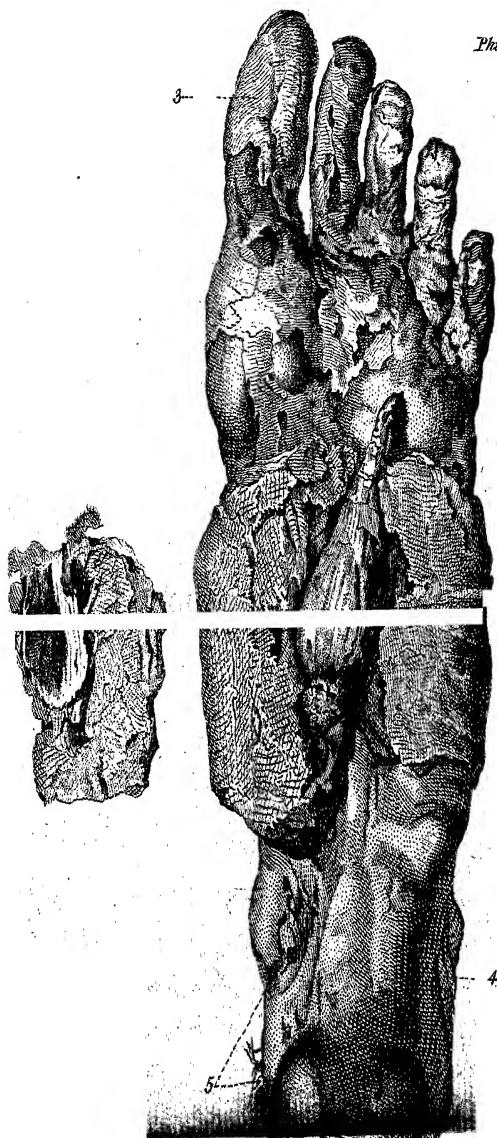
On cutting through the fillets on the thighs, the bones were found invested with a thin coat of pitch; and the filleting was bound immediately on this.

The *tibia* and *fibula* of each leg were found also wrapped in the same manner; and the bones in actual contact with the pitch: excepting in one or two places, where the pitch was so very thin, that the cloth appeared to adhere to the bone itself.

The feet were filleted in the same manner; being first bound separately, and then wrapped together. The filleting had been by some accident rubbed off the toes of the right foot; and the nail of the great toe was found perfect: the last joints of the bones of the lesser toes had been broken away; by which it appeared, that these bones had been penetrated and their cavities quite filled with pitch. The filleting about the heel had also been broken away, and the bones of the *tarsus*, and some of the metatarsal bones had fallen out and were lost; leaving the remaining filleting like a kind of case.

The fillets on the left foot were perfect; except on the heel, and where they had been divided from those of the leg; a small portion of the *tendo Achillis* adhered to the *os calcis*; and some of the ligaments to the *astragalus*.

On cutting into the fillets on the sole of this foot, they were found to enclose a bulbous root. The appearance of this was very fresh; and part of the thin
shining



1. The Bullous root

2. The part of the filling which enclosed the root with the shining skin adhering to it

3. The Spiral lines of the skin

4. The Tendons of *Peroneus Anterior* & *Posterior*
5. Some of the Ligaments of the Tarsus

shining skin came off with a flake of the dry brittle filleting, with which it had been bound down; it seemed to have been in contact with the flesh: the base of the root lay towards the heel. [Vide TAB. I.]

This discovery immediately brought to mind a passage in *Prosper Alpinus* *, and gave some appearance of probability to a relation, which, as he himself insinuates, might give great reason to doubt his veracity. Speaking of the stone image of a *scarabæus*, which was found in the breast of a Mummy he adds: “ *Incredibile dictu, rami roris-*
“ *rini qui una cum idolo inventi fuerunt, folia usque*
“ *adeo viridia et recentia visa fuerunt, ut eâ die a*
“ *plantâ decerpti et positi apparuerint.*”

The fillets were removed from this foot with great care; they were much impregnated with pitch, excepting about the toes; where the several folds united into one mass, being cut through, yielded to the knife like a very tough wax. The toes being carefully laid bare, the nails were found perfect upon them all; some of them retaining a reddish hue, as if they had been painted: the skin also, and even the fine spiral lines on it, were still very visible on the under part of the great toe, and of the three next adjoining toes. Where the skin of the toes was destroyed, there appeared a pitchy mass, resembling in form the fleshy substance; though somewhat shrunk from its original bulk. The natural form of the flesh was preserved also on the under part of the foot; near the bases of the toes. On the

* *Prosper Alpinus rerum Aegyptiarum, &c; cum notis Vesslingii, 1735. pag. 36.*

back of the toes appeared several of the *extensor* tendons.

The root just mentioned was bound to the foot by the filleting that invested the metatarsal bones; no more of this filleting was cut away, than was just sufficient to shew, without removing from its place, a substance which had been preserved in so extraordinary a manner.

On cutting away the fillets which covered the *tarsus*, the bones adhered strongly together; and were covered with hard pitch: with which they seemed thoroughly impregnated.

On cutting away this outward pitch, there appeared very distinctly the tendons of the *peroneus anticus* and *posticus*, the tendons of the *extensor digitorum longus*, and the tendon of the *tibialis anticus*; and besides these a considerable portion of the ligaments of the *tarsus*.

On examining the case formed by the pitch and fillets, which had covered the right foot, and out of which the bones had been taken; there was a very plain mould left, in which there had been enclosed another root similar to that we discovered in the left foot; and in which some of the external shining skin of the root still remained.

During this whole examination, if we except what was discovered in the feet, there were not found the left remains of any of the soft parts.

All the bones of the trunk were bedded in a mass of pitch; and those of the limbs were covered with a thin coat of it, and then swathed in the fillets: which (as has been mentioned) in some places where the pitch was very thin, seemed to adhere to the bone itself.

The

The cavities of many of the bones, on being broken, were found quite full of this substance: the metacarpal bones were so; as were the *radii*, and many others: the ribs, as was before mentioned, were impregnated with it; and so burned, as to be with difficulty distinguished from it: in which state also, were the *vertebræ* and the bones of the *pelvis*.

The pitch had also penetrated into the cellular part of the head of the thigh bone; the small bones of the toes were quite full: but it had not entered into all the metatarsal bones.

From experiment it has been found; that, bones and flesh being boiled in common pitch, it will pervade the substance and fill the cavities of the former: and the latter will be so impregnated with it, as to be reduced to an uniform black brittle mass; not in the least resembling flesh.

This treatment however will not account for the state, in which this Mummy was found; for, if the flesh had not been previously removed, though its appearance would have been entirely changed, yet the filleting could never have been found in contact with the bones.

From this last circumstance it is most likely that the body, excepting the feet, had been reduced to a skeleton, before it was laid up; it is also pretty certain, that it must have been kept some time in boiling pitch; both before and after some of the layers of the innermost filleting were laid on.

The feet seem to have been swathed, at least in part, before they were committed to the hot pitch: and this seems to have pervaded the bandages, the flesh and the bones.

It has been imagined, that the principal matter used by the Egyptians, for embalming was the *asphaltus*; but what we found was certainly a vegetable production. The smell in burning was very unlike that of *asphaltus*; nor did it resemble that of the common pitch of the fir-tree: being rather aromatic.

It was compared with a variety of resins and gum-resins; but seemed not to resemble any of them, excepting myrrh; and that but very slightly.

In all probability, it was not a simple substance; but might be a mixture of the resinous productions of the country, with the pitch of that tree which they had in greatest plenty.

The *Αλειφαρ τῶ Κεδρεῶ* of *Herodotus**, and the *Κεδρεῖα* of *Diodorus Siculus*†, was most probably the tar of the cedar; it is the substance said by these authors to be used for embalming; *Galen*‡ mentions its power of preserving bodies; and § *Dioscorides* calls it *Νεκρὸς ζωή*. *Pliny*, speaking of the cedar, says; that the tar was forced out of it by fire, and that in *Syria* it was called *cedrium*: *cujus tanta vis est, ut in Ægypto || corpora hominum defunctorum eo perfusa servantur.*

Some branches of the cedar were procured from the physic garden at *Chelfea*; and, being treated in the manner described by *Pliny*, yielded tar and

* *Herodot. Euterpe*, pag. 119. ed. Gronov.

† *Diodor. Sicul. lib. i. p. 82.* ed. Rhodomanni.

‡ *Galen. de simpl. Med. Facult. lib. vii. cap. 16.*

§ *Dioscorides de mat. medic. lib. i. cap. 105. pag. 56. Francos. 1598.*

|| *Plinii Histor. lib. xvi. cap. 11. pag. 382. ed. Dalecamp.*

pitch, which had no aromatic finell, and seemed in many respects similar to the produce of the fir-tree. There must undoubtedly therefore have been some other resinous matter mixed with the *cedrium*.

The pitch of this Mummy was carefully distilled; but gave no other produce, than what might be expected from a resinous body; the *caput mortuum*, when burned and elixated, yielded a fixed alkali; to this may be attributed the moisture, which the pitch, that was in contact with the spine and those other parts which were most burned, contracted on being broken and exposed to the air; for this pitch had an alkaline taste, and had been more than melted; having been burned to a *caput mortuum*.

A great variety of experiments were made on this pitchy matter; the result of them all tended to prove, that it had not the least resemblance to *asphaltus*; but was certainly a vegetable resinous substance.

Monf. *Rouelle*, in the Memoirs of the Royal Academy of Sciences for 1750, has given us a very elaborate and ingenious treatise on embalming: wherein he has chemically analysed the pitch of six different Mummies.

From his observations; from what *Pietro della Valle**, and *Joannes Nardius*† at the end of his edition of *Lucretius*, have written on this head; from

* Viaggi di Pietro della Valle, Tom. 4.

† *Lucretius Joannis Nardii de Funeribus Ægyptiorum Animadversio* 50. p. 627. These accounts of della Valle and Nardius are also to be met with in the 3d vol. of Athanas. Kircher's *Oedipus Ægypt.*

what Dr. *Middleton* ‡ observed in the Mummy which was opened at *Cambridge*; from the *Memoires* of Count *Caylus*, in the 23d vol. of *Acad. des Inscript. et Belles Lettres*; and from this present examination; it appears, that various methods of embalming were practised among the *Ægyptians*; and that they used different materials for this purpose: and though *Herodotus* and *Diodorus Siculus* have given us reason to expect to find the bodies in a much more perfect state, than we ever do meet with them, yet, on the other hand, it is evident; from the foot of this Mummy which we examined, and from the account *Monf. Rouelle* and Count *Caylus* have given us in the above mentioned *Memoires*; that all the fleshy parts were not always previously destroyed.

‡ *Middleton's* works, vol. 4. *Germana quædam Antiquitatis monumenta.*

II. *The Sequel of the Case of Mr. Butler, of Moscow, Printed in Philosophical Transactions, Vol. L. p. 19. Communicated by Mr. Henry Baker, F. R. S.*

Read Jan. 19, 1764. **I**N the year 1757 I had the honour to communicate to this Royal Society, from Dr. James Mounsey, F. R. S. late Privy-counsellor and chief Physician to the Empress of Russia, the very extraordinary case of Mr. Butler, a paper-stainer at Moscow, who, on mixing verdegris and false-gold leaf with aqua fortis, by way of experiment, for the production of colours, and stirring the mixture with a small pair of scissars, was soon after affected with a burning pain, first in one finger, then in his whole hand; afterwards in the other hand: in his legs, toes, shoulders, back, belly, forehead, eyes, and by turns in every part of his body, shooting from place to place, like flashes of lightning, as he termed it. He complained likewise of great anxiety and pain at the pit of the stomach, as if a burning iron was laid on it, with many other dangerous and uncommon symptoms, which in about ten days abated, and after a time seemed intirely gone: for the particulars whereof, and the manner in which he was treated by Dr. Mounsey, I must refer to the account printed in the 50th Vol. of the Philosophical Transactions, p. 19.

Thus much being necessarily premised, I now take the liberty to lay before you a letter to me from Dr.

Mounsey, with a farther account of the case of this same Mr. Butler.

Dr. Mounsey's Letter to Mr. Baker.

Dumfries, Mar. 4th, 1763.

Dear Sir,

I AM ashamed that I have delayed so long performing my promise to send you the sequel of Mr. Butler's case, which you thought would be very agreeable to the Royal Society: but I partly waited to see if any thing farther remarkable would follow, and also I was for a long time after that so hurried with affairs, that I scarce had a moment to myself. I remember you wrote me word, that the Society does not much attend to theory and conjecture; so I shall omit to give my opinion of causes and their way of operating, and only send a simple narration of facts, in the same order they happened, extracted from my notes taken upon this occasion.

In my former account of Mr. Butler's case, it is said, that he had recovered his perfect health and strength: yet after that he was often subject to ailments of the nervous kind, and became sensibly affected not only by the smell of paints, but even the handling of some kinds of metallic inodorous bodies gave him anxiety, tremor, faintings, and many other uneasy symptoms.

The handling of verdegris, vitriol, and the like, threw him into these disorders; and he assured me, that the handling copper or iron had the same effect
on

on him. — I often heard his complaints ; but, as I deemed them imaginary, or sensations raised by the apprehension, I oftentimes only strove to undeceive his fancy.——However, I began to see, by some accidents, that there was more reality than I had believed, and that his first accident had left a disposition of the body susceptible to such impressions.

One day having got home a box of Cerussa, he took out some lumps to examine the quality, and handled them without the least suspicion of harm ; but in a few hours after this he was taken with anxiety, palpitation of the heart, and a sense of trembling and weakness of the whole body. He was obliged to go to bed : he took some spirit of Hartshorn, sweated most plentifully, and next day was recovered. Many things of this sort happened to him : but I shall only give you an account of the most extraordinary attack which happened to him June 26th, 1758.

Mr. Butler still wanting to make experiments, but not daring to meddle with the operations himself, directed his wife to make some compositions of blue vitriol, alum, quick-lime, burnt alabaſter, and things of this kind. — They were boiled in six several pots, then let stand some time, and the thin or watry part poured off. She brought these pots to her husband to look at : He was fond to try the colours himself, and, without any apprehension, he took some of those precipitations out of each pot, with the middle finger of his right hand, and rubbed them on grey paper to try the colours. He then put them away, and thought nothing more of the matter, drank tea, and was very well till about three hours thereafter. Then he began to be uneasy, and found pain in his

arms, and especially in his right hand; he became sick at stomach, and felt a trembling over his whole body. He strove to get the better of this attack, and, walked slowly about for some time, but turned pale faint, and fell down. He soon recovered again, and, still thinking to master the illness, drank two or three glasses of wine, which he vomited up again. This began at noon, and at six in the evening I found him in bed frightened and sweating. His pulse was then regular but quick: he was sick at stomach, with anxiety. I ordered him some saline draughts, and plenty of thin warm liquors. In the night he slept but indifferently: his complaints were not continual, but returned by fits, with stretchings of the limbs, tremor, and starting of the tendons over the whole body, and when he began to slumber he was disturbed with frightful dreams of fire.

27th, Early in the morning he observed many small purple spots on his hands. I found them just like purple petechiæ: the most on his right arm, and perceptible through the thick skin of the palm of that hand. There were also some on the other arm and legs, and some of a deeper colour on the thighs, but very few on the rest of the body. As his pulse was now grown quicker, I suspected this to be a petechial fever: but there being no fevers of that kind then in town, and besides, as I could not reconcile the other circumstances, I remained undetermined and much perplexed by all these appearances. About four in the afternoon he was again seized with great anxiety, and pricking burning pains in the feet: the toes were extraordinary red, and he had frequent stretchings. These went almost off in a few hours.

28th,

28th, He was not so much troubled with the frequent returns of his complaints: his pulse was quick, and the spots kept out with itching.

29th, He was much the same as yesterday, only more cheerful in the intervals, and the spots appeared fewer. He got frequently out of bed and walked in the room.

30th, The attacks returned much seldomer, and he would not keep in bed, but walked a great deal about the room, though his pulse was still feverish. Many of the spots disappeared: most were grown pale, and some of a dun hue: Those on the palm of the right hand were almost gone. — He said, the spots grew always fairer every time the fits returned, and then he felt prickling pain with great heat, especially on the insides of his arms and legs, and in his feet and toes.

July 1st, I found him walking about the room, his pulse still quick. Last night he had been pretty easy and free. The spots were pale and disappearing. He took a laxative, which operated very well.

2d, Last night he got pretty good rest, but this morning the prickling and tremorous sensation over the whole body returned, but did not last long. He afterwards got up, walked about, and looked after his business. The spots were mostly gone. He observed, that the prickling pains in his arms and legs, and in a large spot on his back (which troubled him, in all his former accidents) came now only in the forenoons, and then almost ceased for the rest of the day.

3d, Every thing much the same, but the attacks were lighter.

4th, Very little difference; only now and then he was troubled with a glowing painful sensation immediately under the skin, sometimes in one part of the body, sometimes in another, a spot about the bigness of a crown piece.

5th, Things much the same.

6th, The attacks slighter, with the same feelings.

7th, Very little change, but rather better.

Hitherto I was mostly an observer; and, not being forced by an absolute necessity, I did not chuse to load him, at an uncertainty, with many drugs. I had given him little more than absorbent nitrous powders: but now, as he had no fever, but was rather lax and weak, and his nervous system affected, I thought I might begin to give him things more powerful, and therefore ordered as follows:

R. Extract. Cortic. Peruvian. drach. ij.

————— Aurantior.

Gum. Ammoniac.

———— Myrrh. ana drach. j

Sal. Martis drach. ss.

Syrup. Cortic. Aurant. q. s. — F. Pillulæ singulæ gran. iv. deaur. sig. capeat mane et vesperi v. Pillulas.

Here, in prescribing, I had attention to the antipathy nature had shewn to iron, therefore took care the quantity in each dose should be very small, the Sal martis scarcely making two grains.

8th, He took a dose this night, was very restless, and greatly affected with all the former symptoms.

9th, He said, he felt this medicine struggling with the distemper within him; so swallowed, though with great

great reluctance, another dose early in the morning. In less than three hours he was again taken very ill, with anxiety, a sense of trembling over the whole body, and as if prickling sparks were flying out every where.— When I came to him, he begged me to change this medicine, and said it was like to have killed him. Having heard all his complaints, I made the pills be put away, and promised he should have no more of them: but his fear and aversion were so great, that the moment I was gone he ordered the box to be taken out of the house and thrown quite away.

19th, He passed this night tolerably, and found himself much better in the morning: but the complaints came by turns as before.

From this till the 20th, I gave him sundry medicines, but with little more effect than to ease him now and then: for the complaints always returned again in different manners and at uncertain times: but nothing extraordinary happened.

On the 20th, I gave him a dose of Epsom salt, which he had been used to take: it purged very well; but, immediately on its leaving off to work, his body struck out with great numbers of small red spots, without other inconvenience except a little extraordinary heat in the skin.

21st, The spots were almost gone, and he found himself more cool and easy than before.

22d, He took another dose, and the spots returned in the same way more than the first day: he found also the same relief. After this he took more doses of the same salt, always intermitting a day or two. The spots returned, but every time fewer appeared; and at last none appeared on taking these salts.

The

This *sal catharticum amarum* came from England; and whether some vitriolic acid had been used in making it, I do not know; but it is likely there had.

From the first of August he took no medicines; the attacks were grown much less frequent and lighter: only he often felt in the night time like the stroke of an electrified body.

August 13. He was awaked this night by pain, as if burning irons had been clapped to the insides of his legs, with anxiety and a sense of tremor over the whole body. I was sent for, but his complaints were greatly diminished before I came. I found his pulse very quick, irregular and small: but I could find no new cause for the return of his complaints. He had after this some smaller attacks: but in the night of the 23d he was seized with a violent fit of the same sort, with stretchings, and as if prickling sparks were flying continually out of the skin. He had palpitation of the heart, and complained of the want of breath: his left side turned cold, and his right side grew hotter. When I came he was grown better, but the pulse still very small, frequent and inordinate.

24th, He was again attacked in the same manner in the night time, and it also went off in the same manner: but he now grew feverish and kept his bed some days.

By the word *stretchings* I mean the stretching of his body and limbs by a slow and gentle convulsion of the extensor muscles; for in all the attacks I never observed the flexores any way affected. — His feelings were frequently so odd that, he said, he could not describe them. He often felt as if his left side,
from

from his head to his waste was empty, and that millions of small bodies were drove up and down with great velocity: which he likened to the shaking of peas in a bladder.

I tryed many kinds of remedies to rid him of this disorder. He found often relief from them, but the ailments returned again. The remedy I found the most effectual was my putting him on a milk diet, and making him drive hard on a cart every day, forenoon and afternoon, which he continued to do for several weeks. His complaints all decreased; and, when he was threatened with an attack, a few drops of spirits of hartshorn and lavender, or the like, were now of service to him, which formerly had no effect. In short, I gave him again animal food, and he kept his health pretty well.

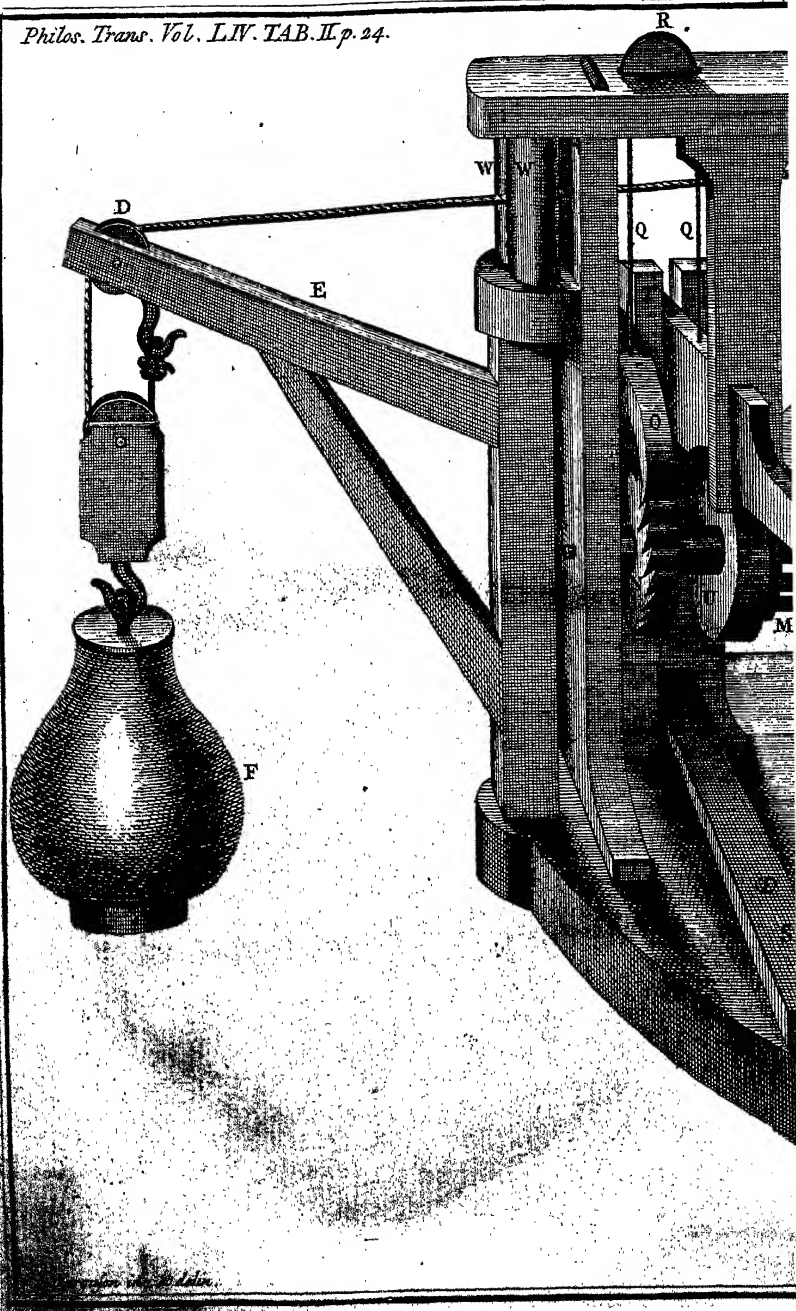
The first year after this he was always fearful; and often complaining, of what appeared to me small things, but by little and little he got the better of these also. Though he always continued to avoid handling metals, minerals, or things painted with these bodies. When I left Russia, he was very well; and I have lately heard by a letter that he continues so; and I believe observes the same circumspection about paints and metals as before.

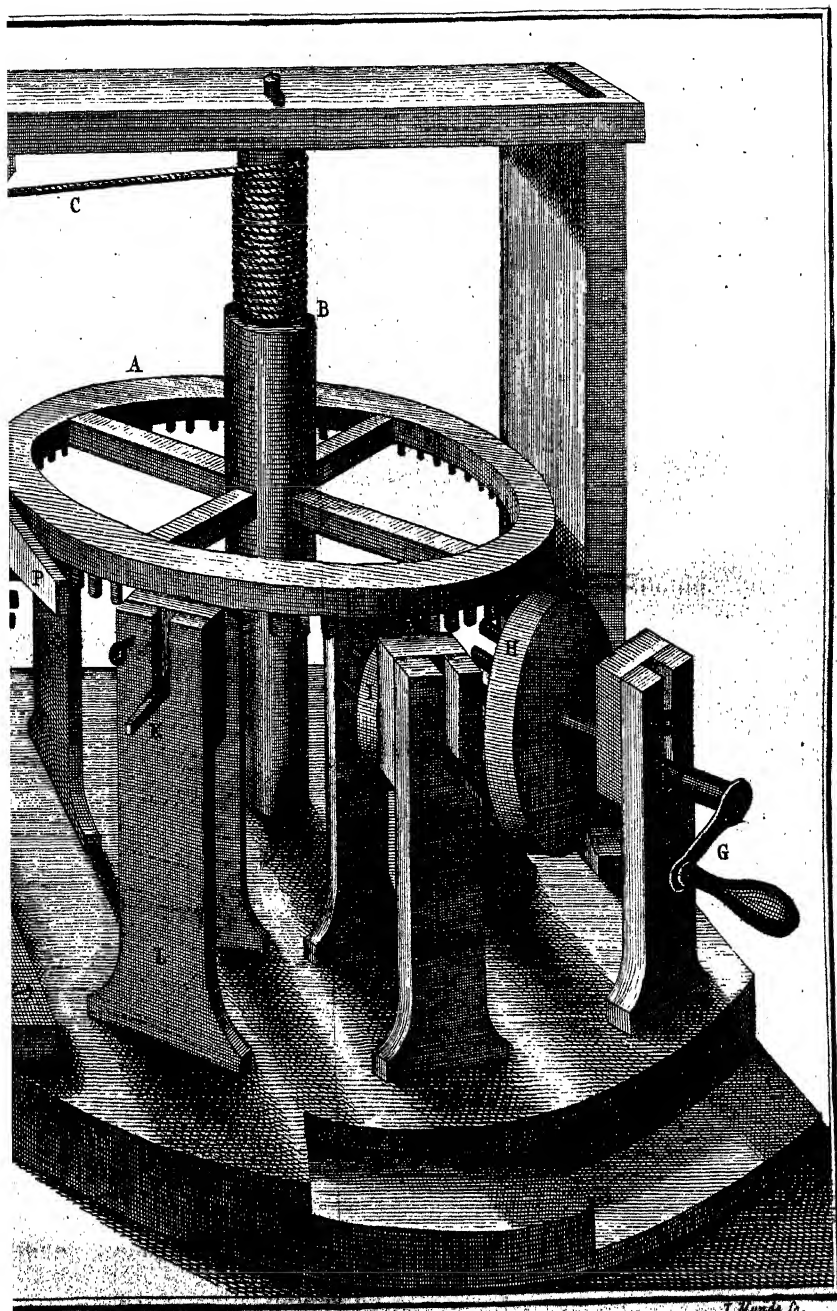
III. *The Description of a new and safe Crane, which has four different Powers; invented by Mr. James Ferguson, F. R. S.*

Read Jan. 19, 1764 **T**HE common crane consists only of a large wheel and axle; and the rope, by which goods are drawn up from ships, winds or coils round the axle, as it is turned by men walking in the wheel. But, as these engines have nothing to stop the weight from running down, if any of the men happen to trip or fall in the wheel, the weight descends, and turns the wheel rapidly backward, and tosses the men violently about within it; which has produced melancholy instances, not only of limbs broke, but even of lives lost, by this ill-judged construction of cranes. And besides, they have but one power for all sorts of weights; so that, they generally spend as much time in raising a small weight as raising a great one.

These dangers and imperfections made me think of a method of remedying them. And for that purpose, I have contrived a crane with a proper stop to prevent the danger, and with different powers suited to different weights; so that there might be as little loss of time as possible: and also, that when heavy goods are let down into ships, the descent may be regular and deliberate.

This crane has four different powers: and, I believe, it might be built in a room eight feet in width; the gib being placed on the outside of the room.





Three trundles, with different numbers of staves, are applied to the cogs of a horizontal wheel with an upright axle ; and the rope, which draws up the goods, coils round the axle. The wheel has 96 cogs, the largest trundle 24 staves, the next largest 12, and the smallest has 6. So that the largest trundle makes 4 revolutions for one revolution of the wheel and it's axle, the next largest makes 8, and the smallest makes 16. A winch is occasionally put upon the axis of either of these trundles for turning it; the trundle being used that gives a power best suited to the weight : and the handle of the winch describes a circle, in every revolution, equal to twice the circumference of the axle of the wheel. So that the length of the winch doubles the power gained by the revolutions of each trundle.

As the advantage gained by any machine or engine whatever, is in direct proportion of the velocity of the power to the velocity of the weight ; the powers of this crane are easily estimated ; and are as follows.

If the largest trundle be turned by the winch, it will make four revolutions for one revolution of the great axle on which the rope coils in drawing up the weight : and as the length of the winch is double the semidiameter of the axle, the power gained will be as eight to one : that is, a man will be able to raise eight times as much weight by means of the engine, as he could do by his natural strength without it ; allowance being made for friction.

If the weight be too great for this power to raise, the second trundle may be turned by the winch, which will turn the wheel and axle twice as slow as

the largest trundle did; because it makes twice as many revolutions for one revolution of the wheel and it's axle: and then the power gained will be as sixteen to one, because the velocity of the power will be sixteen times as great as the velocity of the weight.

If the weight be too great for this power to raise (which we still suppose to be exerted by one man) the winch may be put upon the axis of the third (or smallest) trundle, and then, in turning the winch, the power gained will be as thirty-two to one.

But if the weight should be too great, even for this power to raise, the power may be doubled by drawing up the weight by a double rope, going under a pulley in the moveable block which is hooked to the weight, below the arm of the gib; for then, the power will be as sixty-four to one. If the block has two pullies, and the rope be twice doubled below them, the power will be as 128 to one: and so on, by adding more pullies, according to any required proportion.

Whilst the weight is drawing up, the ratchet-teeth of a wheel slip round below a catch or click that falls successively into them; and so hinders the crane from turning backward, and detains the weight in any part of it's ascent, if the worker should happen accidentally to quit his hold of the winch; or choose to rest himself before the weight is quite drawn up. The catch, in this crane, is constructed much in the same way as in the great crane at Bristol, invented by the late Mr. Padmore, of that city.

In order to let down a weight, the man who works the crane pulls down one end of a lever of the second kind, which lifts the catch out of the ratchet-wheel,

wheel, and gives the weight liberty to descend. But if the descent be too quick, he pulls the lever a little farther down, so as to make it rub against the round edge of a wheel, by which means he lets the weight go down as slowly as he pleases; and, by pulling a little harder, he can stop the weight, if needful, in any part of it's descent. If he accidentally quits his hold of the lever, the catch immediately falls, and stops the whole machine.

In the figure of this crane [TAB. I.] A is the great wheel, and B it's axle on which the rope C coils. This rope goes over a pulley D in the arm of the gib E, and is hooked to the weight F for drawing it up. G is the winch, H the largest trundle, I the next largest, and K is the axis of the smallest trundle, which is supposed to be hid from view by the upright supporter L. M is a trundle, which is turned by the great wheel; and on the axis of this trundle is fixt the ratchet wheel N, into the teeth of which the catch O falls. P is the lever, from which goes a rope QQ over a pulley R, to the catch; the end of the rope being fixed into the lever and catch. S is an elastic bar of wood, of which, one end is screwed to the floor; and from the other end (out of sight in the figure) goes a rope to the farther end of the lever, beyond the pin or axis on which it turns in the upright supporter T. The use of this bar is to keep up the lever from rubbing against the edge of the wheel U, and to let the catch keep in the teeth of the ratchet-wheel. But, when the end P of the lever is pulled down, it lifts the catch out of the ratchet wheel by means of the rope QQ, and gives the weight F liberty to descend:

but if the lever be pulled a little farther down than what is sufficient to lift the catch out of the teeth of the wheel, it will rub against the edge of the wheel V, and thereby hinder the too quick descent of the weight; and will quite stop the weight if pulled hard. And if the man should happen inadvertently to let go the lever, the elastic bar will pull it suddenly up, and the catch will fall down into the wheel, and stop the machine.

W W are two upright rollers, above the axis or upper gudgeon of the gib E: their use is to let the rope bend upon them, as the gib is turned to either side, in order to bring the weight over the place to which it is intended to be let down.

N. B. The rollers ought to be so placed, that if the great rope were stretched close by their outermost sides, the half thickness of the rope may be perpendicularly over the center of the upper gudgeon of the gib. For then, and in no other position of the rollers, the length of the rope between the pulley in the gib and the axle of the great wheel will be always the same, in all positions of the gib; and the gib will remain in any position to which it is turned.

When either of the trundles is not used in working the crane, it may be drawn off from the wheel, after the pin near the axis of the trundle is drawn out, and the thick piece of wood is raised up a little, behind the outward supporter of the axis of the trundle. But this is not material: for, as the trundle has no friction on its axis but what is occasioned by its own weight, it will be turned by the wheel without any sensible resistance in working the crane.

IV. *Of the Moon's Distance and Parallax:*
A Letter to Andrew Reid, Esq; from P.
Murdoch, D. D. and F. R. S. 12 Nov.
1763.

S I R,

Read Jan. 26, 1764. **I** Have at your desire wrote out what I was mentioning to you in our last conversation; of an easy rule for determining the Moon's distance, from the received theory of central forces: which I wish may merit your approbation: it will at least serve as a testimony of the esteem and regard with which I am, &c.

I.

Sir Isaac Newton investigated the law of gravitation, in the duplicate ratio of the distance of the central body inversely, from the following *data*.

1. The length of a simple pendulum which vibrates in one *second* of time, gave him, by Huygens' theorem, a determinate measure of the force of gravity, at the place of observation. And, by his own theory, he could thence infer the like measure for any other place, of a given latitude *.

2. The Earth's semidiameter was computed from the Abbé Picard's measure of a degree of the terrestrial meridian.

* See one of the Essays prefixed to *Busching's Geography*.

3. The Moon's parallax, as determined by the most skilful astronomers, gave him the Moon's distance in semidiameters of the Earth.

4. The time of a periodical month gave him the ratio of the versed sine of the arc of the Moon's orbit which she describes in one *second*, to the radius.

And from these his conclusion was; that the gravitation at the Earth's surface, being diminished as the square of the distance from the Earth's centre increases, would, at the distance of the Moon, produce a fall from rest, in one *second*, precisely equal to that versed sine. Or, that the gravitation of the Moon toward the Earth, being increased as the square of that distance is diminished, would, at the Earth's surface, be of the same quantity as that of falling bodies is (by the experiment of the pendulum) actually found to be.

II.

But the law of gravitation, thus deduced, being found to hold universally, and reciprocally, amongst all the great bodies of our system, so that even the minute anomalies of their motions are explained from it; we may now assume *it* as given, and make the *Moon's distance* the *quantity sought*.

Thus, writing F for the number of feet which a body falling from rest, describes, *in vacuo*, at the equator, in one *second*, V for the versed sine of the arc of the moon's orbit described in the same time, to the radius unity, D for the semidiameter of the equator in feet, and the ratio of the distance of the centers of the earth and Moon, to the semidiameter
of

of the earth, that of X to 1: We shall have, by the general law, the Moon's fall in $1''$, equal to $\frac{F}{X^2}$; but the same fall is equal to $V \times D \times X$; whence $X^3 = \frac{F}{V \times D}$, and $X = \sqrt[3]{\frac{F}{V \times D}}$ is the distance sought, in semidiameters of the equator.

Now a simple pendulum which beats seconds, measuring, at London, 39.126 inches; if the usual allowance is made for the weight of the air, and for the *Newtonian* figure of the Earth *, the weight ($\frac{1}{289}$) taken off by the centrifugal force being likewise restored, a *second-pendulum* at the equator would be 39.154 inches long. And, by Huygens' rule, half this length is to the initial fall in one *second*, in the duplicate ratio of the diameter of a circle to it's circumference: that fall therefore, at the equator, and *in vacuo*, is 16.10185 feet; the logarithm of which number is $1.2068645 = \log. F$.

The *toises* in a degree of the equator, or, which is the same, in a degree of the meridian at *lat.* $54\frac{3}{4}$, being nearly 57200, the logarithm of the number of feet English in the semidiameter of the equator,

that is $\log. D$ will be nearly — — — 7.3211900,
 And the $\log.$ versed sine of the Moon's } — 12.5492882,
 arc in $1''$, being — — — }

Their Sum — — — — — 5.8704782

taken from $\log. F$, leaves $+ 5.3363863$, a third of which is 1.7787954, the logarithm of $X = 60.08906$ semidiameters of the equator.

* See the Essay quoted above.

And the arithmetical complement of this last logarithm, which is -2.2212046 , is the log. tangent of the Moon's mean horizontal parallax at the equator; which therefore, is $57' 12''$, 34 .

III.

Such would be the distance of the Earth's and Moon's centers, were the Earth immoveable: but it is somewhat increased by their revolution round their common centre of gravity.

Writing $x+1$ for that distance, divided by the centre of gravity in the ratio of x to 1 ; imagine a sphere of the same dimensions as our earth, placed at that centre, to exert the same attractive force on the Moon as our Earth actually does, the periodic time remaining unaltered: then must the density of this sphere be diminished in the ratio of x^2 to $\overline{x+1}^2$, that its nearer distance from the Moon may be compensated by the defect of density and attractive force. If now an inhabitant of the fictitious earth were supposed to compute its distance from the Moon, in the manner just now shewn; the quantities V and D would be the same as in the former calculation; but his f would be to our F , as x^2 to $x+1^2$, and thence, his x would be to our X as $x^{\frac{2}{3}}$ to $\overline{x+1}^{\frac{2}{3}}$, that is, $x = \frac{x}{x+1} \cdot \frac{2}{3} \times X$.

This is the distance from the fictitious Earth, or from the common centre of gravity; but (T) the distance from our Earth, is $\frac{x+1}{x} \times \frac{x}{x+1} \cdot \frac{2}{3} \times X$, greater;

as was supposed, in the ratio of $x+1$ to x ; that is,

$$T = \left[\frac{x}{x+1} \right]^{-1} \times \left[\frac{x}{x+1} \right]^{-\frac{2}{3}} \times X = \left[\frac{x}{x+1} \right]^{-\frac{5}{3}} \times X = \sqrt[3]{\frac{x+1}{x}} \times X.$$

Sir Isaac Newton, from the *phænomena* of the tides, estimated the ratio of $x+1$ to x to be that of 40.788 to 39.788. In that case, the cubic root of $\frac{x+1}{x}$ will have for its logarithm 0.0035934; which added to 1.7787954, the logarithm of X computed for an immoveable earth, gives 1.7823888, the logarithm of 60.5883 femidiameters of the equator. And the Moon's horizontal parallax, for this distance, is 56' 44'', 07.

IV.

On the other hand, if we had observations of the Moon's parallax (and distance) which could be reckoned exact enough for the purpose, we might thence determine the ratio of x to 1, that is, the ratio of the quantities of matter in the Earth and Moon.

For having $\frac{T}{X} = \sqrt[3]{\frac{x+1}{x}}$, and $\frac{T}{X} = \frac{x+1}{x}$; likewise T being given from observation, and X computed as above; it is manifest that the ratio of $x+1$ to x , and, by division, that of x to 1, or of the mass of the earth to that of the Moon, is given.

For example, if it should be concluded from good observations, that T , the Moon's mean distance, is 60.5 femidiameters of the equator; for the logarithm of this distance, which is 1.7817554, take the logarithm of X , or 1.7787954, thrice the remainder

will be 0.00888, the logarithm of $\frac{x+1}{x} = 1.02066$; and the masses of the Earth and Moon would, on this supposition, be as 48.4027 to 1.

In all this, a small variation from the law of attraction, arising from the spheroid-figure of the earth, is neglected as inconsiderable; which it will be found to be by whoever takes the trouble to compute its quantity and effects.

R E M A R K S.

1. If F and D were taken of their just quantities, the Moon's horizontal parallax for an immoveable Earth being, at the equator, $57' 12\frac{1}{3}''$, is a *limit* which the true mean parallax cannot exceed: and the correspondent distance 60.08906 is a *limit* which the distance cannot fall short of: both being computed upon the supposition that $x+1=x$, or that the matter of the Moon is as nothing in comparison of the Earth. Nor can the parallax and distance be supposed to lye very near these limits, without leaving too little attractive force in the Moon to raise the tides.

2. If the Moon's mean apparent semidiameter is $15' 38\frac{1}{4}''$, and the distance of the centers 60.5883 semidiameters of the equator, according to Sir Isaac's estimate of the masses; the semidiameter of the Moon will be 0.275601 parts of the semidiameter of the equator, or .2763 of a mean semidiameter of the Earth. And the magnitudes of the Moon and Earth being as the cubes of their semidiameters, if the inverse ratio of their magnitudes is joined to the

direct ratio of their masses (1 to 39.788) the sum will be the ratio of their densities, that of 112143 to 1, a little less than 6 to 5.

3. Supposing still the same semidiameter of the orbit as before, the force of gravity will be to the Earth's attractive force on the Moon as 3570.94 to 1, and to the Moon's force on the Earth as 40.786 times that number, or 149730.4, to 1.

Again, the force of the Moon upon that surface of the ocean to which she is vertical, being to her force on the Earth's centre, as the square of 60.5083 to that of 59.5883; and the difference of these squares being to the latter as 1 to 29.54623, this difference of the forces will support the weight of one 4423968th part of the water at the vertex. And, because the Earth's semidiameter is small in comparison to the Moon's distance, the like differences of force will decrease from the surface to the centre, nearly in an arithmetical progression, as the weight of the water does; making the case analogous to the diminution of gravity by centrifugal force.

But it is likewise easily shewn, that half this quantity of lunar force exerts itself to depress the waters all around at the distance of 90 degrees from the vertex; $\frac{1}{2}$ therefore of the former fraction, that is one 2949312 part of the force of gravity, will be the total cause of the difference in height of the *flood* and *ebb*, in an open and boundless ocean.

Say therefore, if (in determining the figure of the earth) $\frac{1}{2949312}$ of gravity, suspended by the centrifugal force, gave, for the difference of diameters $\frac{1}{2949312}$, what will one 2949312 part give? and the answer, in feet, will be 8.887.

4. In like manner, if we take $8''\frac{7}{8}$ for the Sun's parallax, and, thence, his distance from the Earth 23468,6 semidiameters of the equator, we shall find that his *whole* force to produce a difference of *Flood* and *Ebb*, is to his force at the Earth's centre, as 1 to $7823\frac{2}{3}$. But the Sun's distance being to the radius of the Moon's orbit as 387.34535 to unity, this last force will be to that of the Earth on the Moon, as 387.34535 to 178.7234 (by cor. 2. prop. princip. 1.) And the Earth's force on the Moon is to gravity as 1 to the square of 605883; whence, adding these ratios, the Sun's force to move the sea will be to the force of gravity as the fraction whose logarithm is — 8.1778026 to 1; or gravity is to that force as 13249445 to unity. And therefore, by the same analogy as above, we find the difference of *Flow* and *Ebb*, from the Sun alone, to be 1.97824; one foot $11\frac{3}{4}$ inches.

The solar force therefore, in raising the tides, is to the lunar, as 1 to 4.4924, in a ratio somewhat less than that computed by Sir *Isaac*. The ratio likewise of the sum of the forces to their difference is but 7.869 to 5, instead of 9 to 5, which he assumes from comparing the *spring* and *neap*-tides at *Bristol*. And it is indeed surprizing, how he could, from that *datum*, arrive at conclusions so near the truth, as his very probably are. He tells us he used the ratio of 9 to 5, only till a more certain could be procured, And therefore the foreign mathematicians, who have censured him on that head, and on some other articles of this doctrine, might have spared their reflexions; at least till they could shew that their own deductions were more agreeable to nature and observation.

6. Unity representing the force of gravity, & the Sun's distance, the Earth's force on the Sun will be $\frac{1}{d^2}$, or the fraction whose logarithm is -9.2590218 . And the solar force on the Earth is (from the numbers in remark 4) to the force of gravity as 1 to 1673.1: whence the attractive forces (and masses) of the Sun and Earth will be as 325172,3 to 1. Add to this the inverse ratio of their magnitudes, collected from the Sun's mean apparent semidiameter $16'.6''$, and the parallax $8''\frac{4}{3}$; and the density of the Sun will be to that of the Earth, as 1 to 4.068.

All this upon the supposition that the masses of the Earth and Moon are as 39.788 and 1. Hereafter, when the Moon's distance shall be more certainly known, that element may be corrected, and the operations repeated.

As to the Sun's parallax $8''\frac{4}{3}$, it cannot be much affected by any future determination of the Moon's distance. Nor is it here assumed of that quantity, at random; but from a theorem deduced from the established principles. I am, however, too diffident of myself to communicate it at present: because, altho' it agrees very well with Mr. *Short's* conclusion from the *Transit of Venus*, it differs considerably from that which a very learned and justly celebrated author hath lately published.

Note, The periods, as assumed in this paper, are;
 a *sidereal year* of 365.2563923 days; the periodical
 month 27.32165835 days.

V. *An Attempt to Account for the Origin and the Formation of the Extraneous Fossil commonly called the Belemnite. [Vide TAB. III. IV. V.] By Mr. Joshua Platt.*

To the Right Honourable George Earl of Macclesfield President, and to the Council and Fellows, of the Royal Society, the following Attempt to account for the Origin and true Formation of the Extraneous Fossil commonly called the Belemnite, is humbly addrest, by

Their much obliged and obedient

humble Servant,

Joshua Platt.

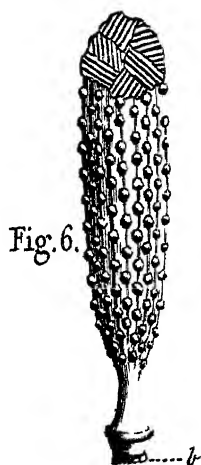
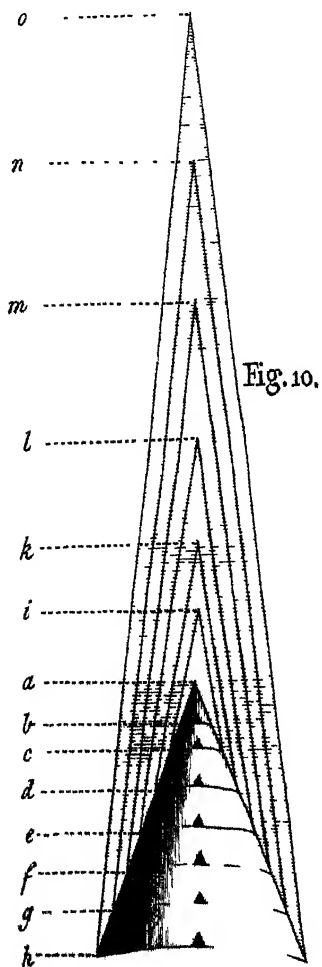


Fig. 11.

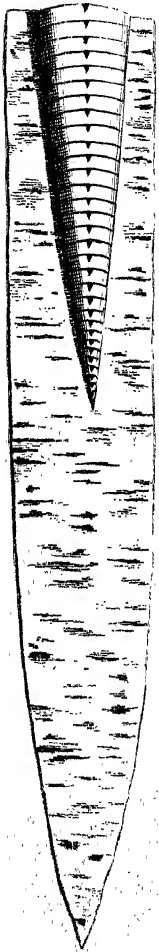


Fig. 5.



Fig. 1.





Fig 12



Fig. 13.

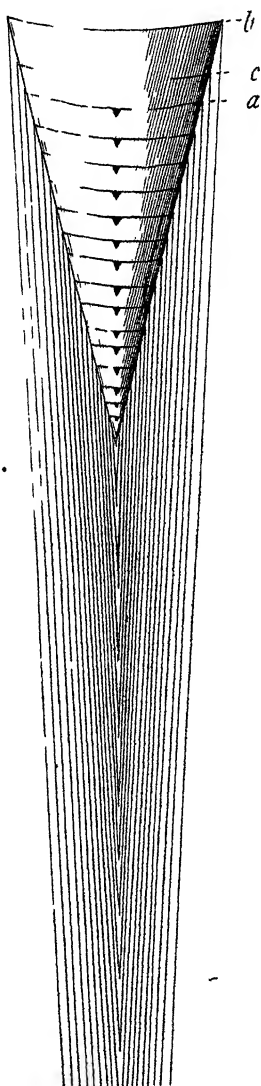


Fig. 14.

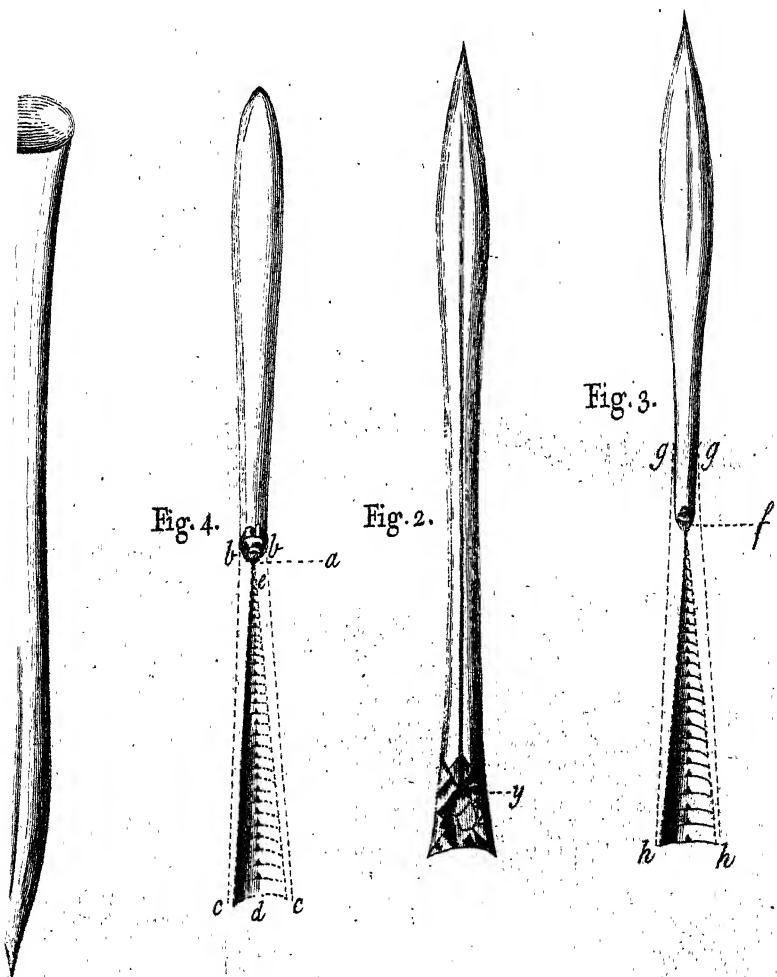


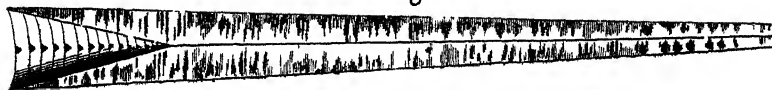
Fig 17



Fig. 15.



Fig. 8.



Found at Headington Stone pit near Oxon

Fig. 7.

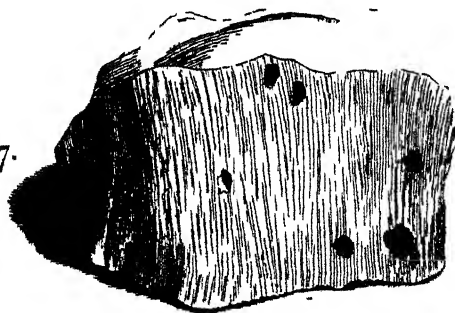


Fig. 16

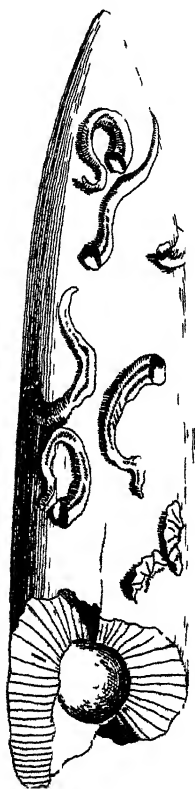


Fig. 9.



Read Jan. 26,
1764.

THE public hath of late been agreeably entertained with descriptions of many curious Fossils discovered in different parts of this kingdom: but very little hath been offered with a view to ascertain their origin and formation; a point of much greater importance to a curious mind, than the most accurate descriptions, or the neatest delineations. It may indeed be thought unnecessary at this time, to say any thing of the origin of extraneous Fossils in general; all our modern naturalists being fully convinced, that they are the exuviae or remains of animals and vegetables, and the greater part of them of marine production.

But as to their particular origin and formation; in what manner they were produced in the recent, and how and with what matter they afterwards became impregnated in their fossil state; all this is a field of natural inquiry, that has been very much neglected, notwithstanding it is the most fertile and productive of useful and entertaining knowledge. Besides, were we to consider it in this view, the recent and fossil remains would be found to throw a mutual light upon each other, and the naturalist would not be so often at a loss to class every new fossil acquisition, of which the recent specimen is not to be found, especially whenever the fossil has any thing seemingly æquivocal in its formation, so as on a superficial inspection to render the matter doubtful, whether the body belongs to the animal or vegetable kingdoms, or indeed to either of them. One of the first note is the Belemnite, which has not until very lately been even ranked amongst the marine productions;

but

but whose origin and formation have never yet been fully explained. I shall not enter into a minute detail of the several species of the Belemnite. The history of this extraneous fossil, or an attempt to account for the origin and formation of the Belemnite, so far as they can be discovered and confirmed by reasonings drawn from facts and experience, is the object of the present enquiry. I shall therefore confine myself to two species of the Belemnite; the one common in most counties of this kingdom, and vulgarly known by the name of Thunder-bolt [*a*]: the other that of the fusiform or Spindlekind [*b*], found in slate-stone at Stons-field, but in far greater plenty in the clay near Piddington [*c*] Oxfordshire; and in the chalk-pits of Kent and Surrey [*d*]. Those in chalk have been often mistaken for spines of the sea-hedgehog, or *Echinus Ovarius*; but the characteristics of these two bodies are widely different. The Belemnite breaks in a direction perpendicular to its axis [*e*]: the spine obliquely [*f*]. The Belemnite, when broken, exhibits central rays; the Spine a smooth resplendent surface. This distinction is invariable, if the trial be repeated a thousand times. These different appearances are probably the effects of different formations: and therefore the Belemnite seems to be formed by apposition, and the Aculeus or Spine by protrusion, or, as Mr. Reaumur calls it, by intus-susception. The radii in the Belemnite are owing to the fine laminæ, of which it is composed; they are so very thin, and break so nearly alike, that they have ever an horizontal surface when broken,

[*a*] TAB. III. Fig. 1. [*b*] Fig. 2. [*c*] Fig. 3.
 [*d*] Fig. 4. [*e*] Fig. 5. [*f*] Fig. 6.

which

which is common to all the shells of the trichite kind [g]. The spine being formed by protrusion, its component parts are adjusted on a different manner, and the pores like the cancelli in bones (though not so distinct) are irregular, which is the reason of its breaking obliquely in any direction, but it is generally smooth by being saturated with a plated kind of spar [h].

My ingenious and very worthy friend Mr. Brander, in a dissertation on the Belemnite, presented to the Royal Society [i], justly observes; “ that the Belemnite belongs to the testaceous part of the animal kingdom, and to the family of the nautili.” And I would beg leave farther to add, that this gentleman’s sentiments are greatly strengthened by the surprising analogy, which the Belemnite bears to the little pearly concamerated shell, or cornu Ammonis; and the orthoceratites, to the large nautilus; the former having its siphunculus upon the verge, as the latter has it in the center of the diaphragm, or partition, of each cell or chamber. “ It has indeed been truly matter of speculation (continues Mr. Brander) how this huge solid substance called the Belemnite, exclusive of the nucleus, could be formed; and how it happens, that some Belemnites should have the nucleus within them, others not; the cavity to

[g] Fig. 7. A piece of the Penna Marina, perforated by the Pholades.

[h] Spar seems to be nothing but crystal debased by a calcareous earth: the more debased sort breaks in a hairy trichite manner, the more pellucid kind with a smooth surface; and always in an oblique rhomboidal direction; which perhaps may in some measure enable us to account for its double refraction.

[i] Philosophical Transactions, Vol. xlviii. for 1754, page 803.

contain the same in some very small, in others scarce or not at all visible".— But I flatter myself, that it will be found upon enquiry, that these are only circumstances, which are common to other testaceous bodies, that have been accidentally broken or decayed by time, when forsaken by their inhabitants. For no testaceous body can be formed without an inhabitant; nor does it appear to me, that any Belemnite was ever formed without an alveolus, or concamerated shell.

The conical cavity and its nucleus are always proportioned to the bulk of the Belemnite, but not to its length: some are four times longer in proportion to the alveolus than others. The apex of the conical cavity, where the alveolus is first formed, in some runs up about half the length of the whole Belemnite; in others not a sixth part of the whole [*k*]: but the aperture, or upper chamber [*l*] is equally proportionable to the bulk, or circumference of the Belemnite, of whatsoever size or shape; and is the seat [*m*] or dwelling-place of the animal, that forms the Belemnite. In what manner this work is executed, I shall now endeavour to explain.

A considerable part of marine bodies, especially those of the testaceous tribe, are generally buried in mud or sand, except some few, which stick to rocks, &c. as the limpets and periwinkles; by which means we are prevented from making those remarks upon the several stages of their growth, which an accurate enquirer would desire. We must therefore have re-

[*k*] Fig. 8.

[*l*] Fig. xiii. *b. c.*

[*m*] We never find a Belemnite with part of the alveolus, but the vestigia or marks of the remainder appear in the cavity, and are continued to the verge of it.

course

course to the different steps or periods of their life and growth, as they are marked out by the indented lips or foldings of the shell; untill they arrive at their full size; when they begin to fortify themselves, by bulwarks and strong holds, against the injuries and incidents, which attend old age. This is most conspicuous in the cowree, or concha Veneris of Lister, book iv. sect. 9.

Mr. Reaumur [n] found, by repeated experiments, that land snails form their shells by juxtaposition: as the animal grows in bulk, the shell is increased by a mucous matter emitted from the body of the animal, which hardens by degrees into a testaceous substance: and from the experiments upon land shells that great naturalist concludes by analogy, that all testaceous bodies are formed in the like manner, particularly those of the turbinated kind.

To this general rule an objection is made by Mr. Poupart, from the formation of the cowree, or concha Veneris before mentioned: but this learned gentleman was not aware, that this shell is first a buccinum, forming many convolutions before it draws in the verge to form the indented lip.

It was this very objection of Mr. Poupart, which led me to examine into the growth of the cowree; and by sawing one of them through the middle, I found a turbinated shell within the outer wall, consisting of six or seven convolutions, but no stages, or periods, of the indented lip appeared in any of the convolutions, as we find in the helmet shell, and several of the buccinæ. I then began to consider how this animal enlarged its dwelling; and was fully convinced, that

[n] See his book of insects.

no more convolutions could be carried on; the indented lips being a full stop to its inward dimensions; and that here was the period of its growth. My sentiments were just as to its inward dimension; but observing that the lips of some were much larger than others, and that the curved part of the outer lip appeared thicker, when sawed open, than the other parts of the shell; I began to think, that the animal, instead of enlarging the inner dimensions, was employed in thickening the outer wall, to guard against injuries and accidents, so common to the inhabitants of that turbulent element the sea. I was the more confirmed in these sentiments by seeing the beautiful spots, with which this animal decorates its house, covered by other spots of different colour and size, as new laminæ were added to strengthen the last-formed convolution. It is really matter of admiration to see how these shells are adorned and variegated; the exquisite polish, which covers the whole, infinitely surpassing the skill even of the most accomplished human artist. These new coverings or laminæ, which are carried from the lips, terminate in the middle of the back part of the shell; and there form a list, or seam, of a quite different colour from that of the other part of the shell, and of an unequal surface.

This very circumstance gave birth to my sentiments concerning the formation of the Belemnite: for whoever considers the seam or fulcus in the Belemnite, will, I think, conclude with me, that the outward lamina is formed latest, as in the cowree, and that the seam or fulcus is caused by the several additional coverings or laminæ terminating there. But as the anatomist makes fresh discoveries by dissecting the
subject,

subject, so (if I may be allowed the comparison) I received farther information by luckily meeting with a Belemnite, whose laminæ were in a manner dissected and laid open by the vague acid, or some other corroding menstruum, which every where pervades the earth, destroying some bodies [o], and forming others [p]. The laminæ of this truly wonderful body are here exposed to view [q], and plainly shew us, that nature, in this, as in all her works, pursues the most simple, easy, and shortest methods, though they appear ever so intricate and interwoven. This specimen will, I hope, serve to explain a matter, which hath so long puzzled the curious in natural history; and convince us, that there is nothing more wonderful in the formation of the Belemnite, than in that of a cockle, oyster, or any other testaceous substance; with this difference only, the oyster strengthens its shell, and excludes its first habitation, by additional laminæ formed *within*; the Belemnite incloses its dwelling by adding new laminæ *without*. Figure X. represents the Belemnite split up the middle, with the siphunculus in the front: *a, b* exhibit the first formed cell, or seat of the animal ab ovo. As the animal grows larger, it forms a second cell or chamber *b* to *c*, at the same time covers the first cell, by forming the appendage or guard *c, i*, which is the first stage of the Belemnite. In forming the third cell *c, d*, fresh laminæ or coverings are carried on from *d*, to *k*; and so of the rest, *e, f, g, h*; or *l, m, n, o*. When we have duly considered the manner, in which the shell is

[o] All calcareous substances.

[p] Such as selenites, pyrites, marcasites, talk, gypsum, &c.

[q] Fig. 9.

thus formed; it will be no difficult task to account for the different sections and broken parts of the Belemnite; and in what manner they were reduced to the several forms or appearances, in which we commonly find them.

The better to illustrate my conjecture, I shall first exhibit some drawings, which shew the several specimens broken and imperfect; and then propose my sentiments concerning them before they were deserted by their inhabitants. Figure 6. shews the spine of the echinus ovarius broken obliquely, as is common to all of them. Figure 5. exhibits the inward structure of the Belemnite, when broken horizontally, with the central rays. Figure xi. is the same Belemnite split through its axis. Figure xii, and xiii, are broken in the same direction as Fig. 5. and xi, and shew how the several laminæ [*r*] are placed one over another in the manner, in which it is formed. Fig. xiv. shews the Belemnite in the most perfect state we ever find it. Fig. 4. is the fusiform Belemnite found in chalk, which has been often taken for a spine. *a*, which is the termination of the conical cavity, has been thought to be the *socket* of the spine, which receives the papilla, when growing to the echinus; but, when compared with the socket of the true spine [*s*], we find it widely different. The pricked lines *b*, *c*. *b*, *c*. shew what the fusiform Belemnite was, when perfect, with the alveolus *d*, *e*. Many of

[*r*] These distinctions of the laminæ I presume to be owing to the mineral steams insinuating themselves into the Belemnite, when the spar pervaded the pores, and destroyed the texture, but retained the true form by substituting its self, and filling the Plasm or mould of the Belemnite.

[*s*] Fig. 6. *b*.

I

those

those found in chalk seem to be somewhat injured at the end *a*, where they are deficient, and are rounded, but have an uneven surface, as if they had been gnawed or eaten by the pholas. Those found in clay near Piddington [*t*], Oxfordshire, approach nearer to the fusiform kind, and have a different appearance at the smaller end *f*, where the laminæ are reduced to a white impalpable powder, by corrosive juices in the earth, so as to stain the fingers when first taken out; and they afterwards retain a white chalky appearance: but, amongst a great number, I never found one that was three inches long. These have suffered in the same manner as Fig. iv. Fig. 3. *f*, shews where the alveolus terminates: *g*, *b*. *g*, *b*. how much has been destroyed by vitriolic acids [*u*]. At Stons-field they are found much longer than at Piddington, and are inclosed in stone, which is split by the workmen to make slates. Here we often find

[*t*] Fig. 3.

[*u*] It may be asked, why one part suffers more than another, as all parts are homogeneous, and free from extraneous mixtures? My answer is; because those parts, where the concamerated shell is lodged, are much thinner than the other parts of the Belemnite; and consequently the walls are more easily broken down, and the alveolus, being still less solid, is sooner destroyed, and reduced to an impalpable powder, by vitriolic and other acids, which the water takes up as it passes through different strata, abounding more or less with pyritical matter. Where no spar follows the acid, the parts are carried away and lost in the interstices of the earth, and a mould or plasm is left, which Steno calls an aerial shell. See his Prodomus, pag. 84. But where the spar abounds, it pervades the whole substance, fills up the cavity, and assumes the true form of the shell; and sometimes, by bursting the pores, is so far substituted in the place of the original particles, that the several diaphragms, with the siphunculus of the alveolus, are accurately and nicely preserved.

them.

them in a much more perfect state [w] than the former, with the alveolus in many of them; but that part is commonly crushed [x] by the incumbent matter.

The siphunculus of the Belemnite is always upon the verge of the chamber, or cell; and in the siphunculus is a little gutt or ductus, proceeding from the body of the animal, by dilating or contracting of which the animal, it should seem, may go out or into its cell at pleasure. This is the only stay, which the animal has to secure its retreat: but I cannot agree with the learned doctor Hooke [z], *that the gut or ductus passes through all the cells to the end of the spiral cone*, either in this shell or the nautilus. His discovering of a spiramentum in the center of the latter was merely conjectural; for the ends of the spiral cone of concamerated shells [a] are shut up in the same manner with those of the turbinated kind: and it is common for all turbinated shell-fish, as they increase in bulk, and enlarge their shells, to leave their bottom or first-formed convolutions. Therefore I make no doubt but the same is done by the concamerated tribe; for if the gut go through only one or two valves, it will be a sufficient stay to the animal, and, being contracted or dilated, will serve all the purposes above mentioned. How far this is practicable by our little inhabitant, cannot absolutely be determined; but if it be constantly fixed by the gut to the siphunculus, it has a surprising power of contracting and dilating its body, to extend so far as the bottom or point of the Belemnite,

[w] Fig. 2.

[x] Ibid. at y.

[z] Hooke's posthumous works published by Derham 8°. p. 306.

[a] See the little pearly cornu-ammonis shell.

which.

which in some is more than thirty times the length of the cell, into which it returns [b]. I am apt to think, that this gut or ductus, as well as the body of the creature, is capable of being extended very considerably, to serve all the uses of forming the Belemnite, without leaving the siphunculus; and that the gut serves for the same purposes with the tendons of the oyster; the latter to open and shut the shell; the former to allow the animal to go out and in at pleasure. And as the oyster feeds altogether in the shell, by opening the verge, the Belemnite (whose residence is in the great deep, which is seldom disturbed) very likely goes out in quest of food, but travels only upon the guard or rampart, leaving a trail behind, as all land snails do; which hardening into a testaceous substance, increases the dimensions of the outer walls, both in length and thickness, from the cell or chamber, to the bottom or point of the whole Belemnite. The animal in its progress and return clasps the whole guard, as a snail does a small branch of a tree in the gardens; and where the two sides meet, there the sulcus is formed, as is evident from the laminæ in Fig. 9.

The Belemnites, like all other testaceous bodies, have the vermicular tribe attached to them, and are perforated by the pholades. Other marine bodies also affix themselves to the Belemnites, oysters in particular: but this never happens whilst the animal inhabits the shell, because the new additional laminæ would so cover the affixed body, and also the cells of the pholades and vermiculi, that they could have no communication with the water, and must consequently

perish. These bodies, thus attached, are the strongest proof we can desire, that the Belemnite is of marine production. Indeed it may be objected, that the bones of quadrupeds, wood, and stone have these bodies adhering to them, and therefore may be said to be marine, as well as the Belemnite. But when we bring them to chemical trial, the objection vanishes; for the bones either come out of the furnace with a black core, or they are reduced to ashes; whereas the Belemnite is changed into a fine calx, after the manner of all testaceous bodies, and is converted into a species of phosphorus [c]. The oysters, having no loco-motion, frequently affix themselves to other bodies, that they may be better able to stem the tides, and currents, which might otherwise carry them from their proper beds, and places of feeding. This attachment to other bodies no way incommodes them, because they increase the dimensions of their shells by adding fresh laminæ inwardly: the first formed laminæ, being, as it were, excluded, lie in the manner of tiles upon the roof of a house, and exhibit the several steps or stages of their growth.

I believe a Belemnite is very rarely found perfect in the fossil state: those in gravel-beds [d] have suffered very much by being rubbed against stones, &c. by the fluctuating waters: those, which we find in rubble at Garfington-pitts [e], have many adventitious bodies adhering to them, and consequently were deserted by

[c] The Belemnite after calcination, has all the properties of the Bolognian stone. If it be exposed a few minutes to the Sun, and immediately taken into a dark room, it will shine like Phosphorus for some time; and when the light diminishes, if again exposed to the Sun, its splendor will be renewed.

[d] Fig. xv.

[e] Fig. xvi.

their inhabitants before they rested there. In the clay at Shotover [*f*] near Oxford, they have a curious smooth surface, but are otherwise imperfect: at Stonsfield, in the slate-stone, they are generally crushed [*g*]: those approaching nearest to perfection, which I have seen, came out of the sand [*b*], under the bed of stone at Garfington-pitts near Oxford: the outer part is quite perfect, and the verge of the conical cavity is as thin as paper, but the alveolus is destroyed, except the apex or point. At Thame, in digging for stone, several small ones were found in a stratum of blue clay of a more cylindrical form [*i*]; some of which have the pearly substance still remaining; an incontestable proof of their being marine productions.

How much of the cavity is occupied by the alveolus, cannot be truly ascertained, until a perfect one can be found, which it will be hard to do in the fossil-state; but if we may judge from the nautilus, the walls are carried to a distance from the last formed valve, much greater than that, at which the valves are placed from each other; as in fig. xiii. from *a*, to *b*, which gives the animal all the convenience of forming a new valve or diaphragm, *c*. This circumstance has been very ingeniously cleared up by a learned physician in one of our monthly papers [*k*].

The recent nautili are very common in the eastern seas; and in the fossil state are frequently found with the Belemnites at Garfington near Oxford. Why may we not therefore expect to find a recent Belemnite,

[*f*] Fig. i.

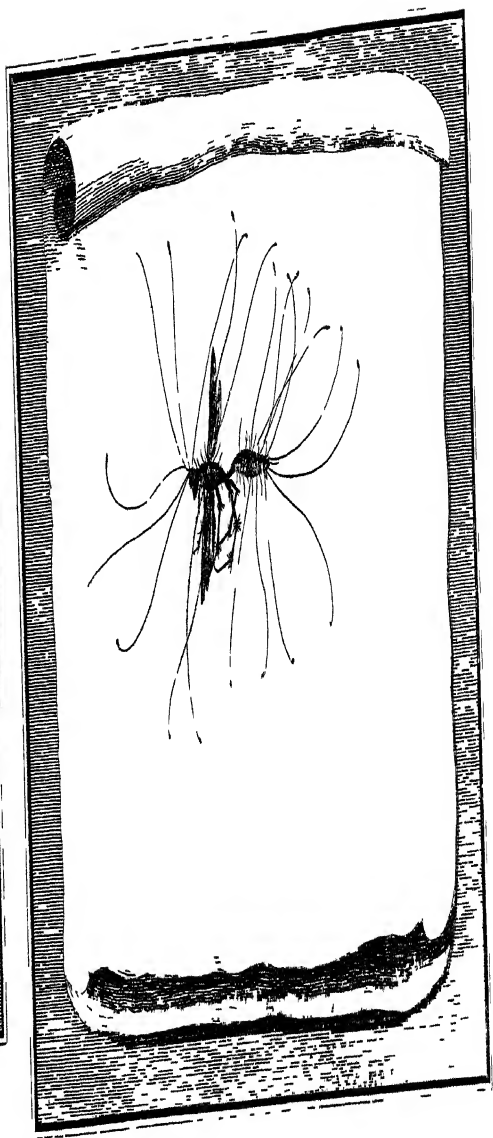
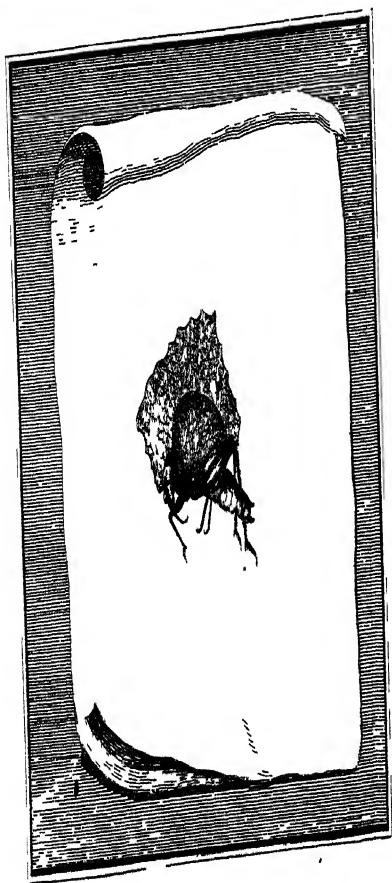
[*g*] Fig. 2. y.

[*b*] Fig. xiii.

[*i*] Fig. xvii.

[*k*] Gent. Magazine for Jan. 1752. pag. 8.

as well as a recent nautilus, if a diligent person were strictly to examine the coasts, where the nautili are found? Persons commissioned to collect shells, and other curiosities, generally grasp at such things as feast the eye; not regarding those of less beauty and lustre, which would help us in our researches, and greatly illustrate the more useful parts of this study. A premium, offered by a Society of Vertuosi, might encourage our sea-faring gentlemen to search the several coasts, upon which they touch in their long voyages, and to use drag-nets as they do in drudging for oysters: from such assistance, variety of new subjects would be produced; and great improvements might be expected, if their inquiries should be successful.



I. *An Account of a Singular Species of Wasp and Locust: By Samuel Felton, Esq; F. R. S. in a Letter to Mr. Henry Baker, F. R. S.*

Dear Sir,

King-Street, Covent-Garden,
December 2, 1763.

Read Feb. 2, 1764. **T**HE honour I received, by being elected a fellow of the Royal Society, excites me through gratitude to offer that learned body whatever occurs to me new, or worthy attention in the animal world; and the respect I bear you, dear sir, for your learning and goodness, to which I must add your having been so useful a member of that learned body such a number of years, and the encouragement you have constantly shewn towards promoting natural history, emboldens me to transmit to you this paper containing the descriptions of a very singular species of Wasp and of Locust, [TAB. VI.] which I met with in the Island of Jamaica. I made what search I could in the natural historians, but cannot find that they have ever been taken notice of, therefore are as yet unknown to the learned, or non-descripts. I therefore offer them by your means to the inspection of the Royal Society, to be inserted in the Transactions, if deemed worthy their attention. I beg leave to accompany these descriptions not only with the subjects.

subjects themselves, but also with accurate drawings to compleat their history, and am, with great esteem,

Dear Sir,

Your most obedient

And most obliged humble servant

Samuel Felton.

I. CRINITA — *Vespa fetis colli thoracis abdominisque radiantibus corpore longioribus.*

Large as a common Wasp, but rather narrower.

The head is brownish, the vertex black, in a triangular form.

The Antennæ are shorter than the thorax, a little thicker towards the end, of a yellow brownish colour; but black in the middle.

The thorax is light brownish on the back, but on the sides and underneath black: before the insertion of the wings, there are two yellow lines running transversely downwards; just over the insertion of the wings two hairs go out on each side of equal length, and very near twice as long as the whole body; from the upper part of the neck likewise go out two hairs as long as the body.

The Abdomen is divided into six segments of which the first is very narrow at its basis, quite black, only the hind margins yellow; from this segment there only grow out two hairs twice as long as the abdomen, at the base but no where else; the other
five

five segments are betwixt brown and yellow coloured, their hind margins a little paler, and the second has a black girth near the fore margin; hairs go out near the fore segment as rays; in the second only three, and they shorter than the abdomen, especially the side ones; in the third, fourth and fifth segments, there are four or five long hairs longer than the body, and several shorter ones, especially underneath where there are no longer ones; the sixth segment is terminated with a long hair.

All these hairs are of a light brown colour, seem to be stiff, but their ends are quite soft like papillæ, and from thence thicker.

The wings are shorter than the abdomen; the upper ones folded.

The legs are black, except the thighs which are yellow; at their joints there are short hair like rays, whose ends are likewise short and thickened.

II. RHOMBEA CICADA thorace compresso membranaceo foliaceo subrhombeo postico latiore.

The thorax is like a leaf that is raised perpendicularly from the body; it is three times as broad as the body, but the same length. This leaf is very near of a rhomboid figure, a little broader, or rather higher over the back; it is membranaceous, probably brownish, (when alive) half pellucid, with two spots that are more pellucid, or transparent; the larger one is very near the middle, but the smaller lower; the margins are waved, especially towards the hind angle; over the forepart of the body the leaf is double.

The

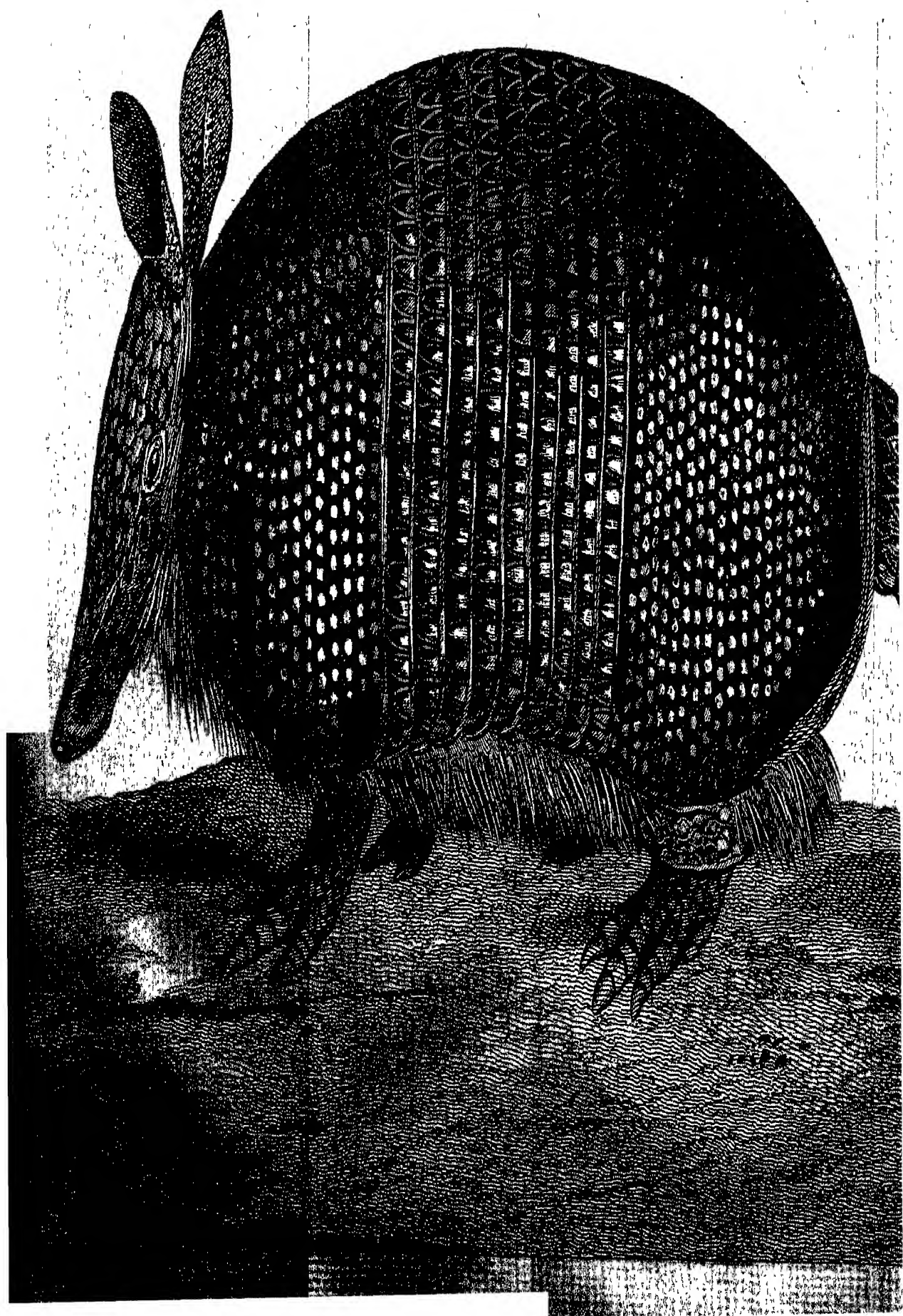
The abdomen is a little longer projected backwards than the leaf of the thorax.

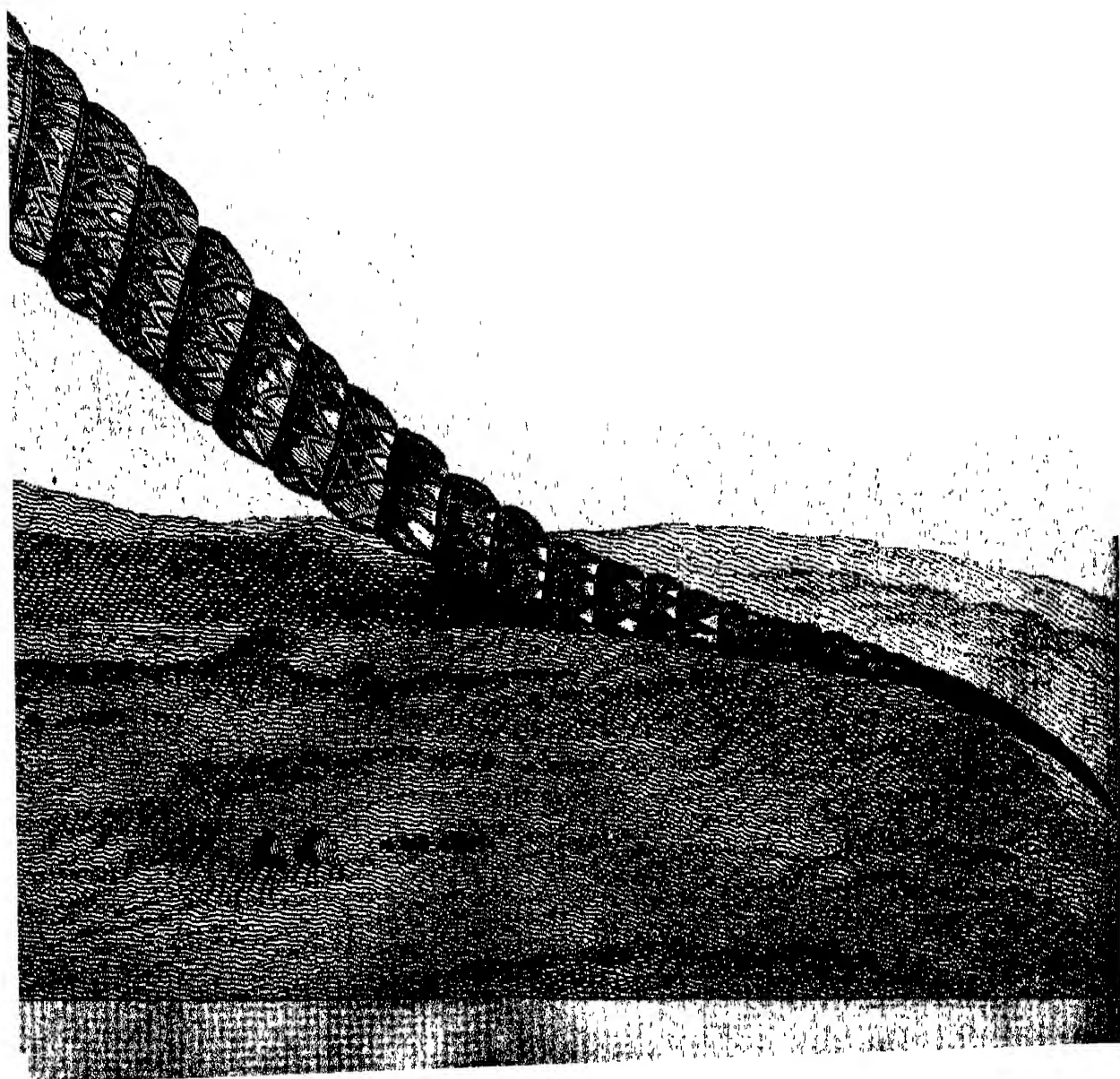
The insect had not yet got its coleoptera and wings.

The hind thighs that are thicker have on the upper side an additional narrow membrane added to them.

The head and maxillæ are very like those of the gryllus's, but there is such an affinity between this and the *Cicada foliata* Linn. syst. nat. 435.6. that I should think it the same species, if the thorax of this was not broader behind towards the end.

The antennæ are broke off; else from their length one might learn, to what genus the tribe Linnæus calls *Cicadæ foliaceæ* (syst. nat. p. 435.) should be referred; for I am in doubt whether Linnæus ever has seen perfect specimens of them.





VII. *An Account of an American Armadilla: By William Watſon, M. D. F. R. S.*

To the Royal Society.

Gentlemen,

Read Feb. 9. ¹⁷⁶⁴ **I** Herewith lay before you, for your inspection, the drawing, by the ingenious Mr. Paillou, of an animal very ſeldom, if ever, ſeen alive in England [TAB. VII.]. It is now alive in excellent health, and in the poſſeſſion of the right honourable the Lord Southwell. It is called by Linnæus, in his *Systema Naturæ*, *Dafypus cingulis novem, palmis tetradactylis, plantis pentadactylis*.

Marcgrave and Ray have both deſcribed it under the appellation of *Tatue Braſilienſicus*. Albert Seba has likewiſe deſcribed it in the firſt volume of his elegant and elaborate *Muſeum*. He calls it *Tatou*, five *Armadillus Americanus*. The figure attending his deſcription is taken from a dead animal. The drawing therefore is hard and ſtiff, and the colouring does by no means come up to the living animal.

This creature, which is called by naturaliſts the American *Armadilla*, was brought hither a few months ſince to Lord Southwell, from the country near what is uſually called the Moſquito ſhore, upon the American continent. Its weight is ſeven pounds

avouirdupois, and its size that of a common cat. It is a male, and has improved greatly both in appearance and colour, since it has been in his Lordship's possession. It is fed with raw beef and milk, and refuses our grain and fruits. In its own country, according to the accounts of those who treat of it, it burrows in the ground.

As there is no good figure of this animal existing in any of the authors, who have treated of it; and as Lord Southwell has been so obliging as to permit me to have one taken of it by the before-mentioned able artist; I had reason to believe, that the inspection of it would not be disagreeable to the Society. I am, with all imaginable regard,

Gentlemen,

Feb. 9, 1764;

Your most obedient,

humble Servant.

W. Watson.

VIII. *An Account of the Quantity of Rain fallen at Mount's-Bay in Cornwall, and of the Weather in that Place: In a Letter from the Rev. William Borlase, M. A. and F. R. S. to the Right Rev. Charles Lord Bishop of Carlisle, F. R. S.*

My Lord,

Ludgvan, Oct. 31, 1763.

Read Feb. 16,
1764.

A T Carlisle, I find by your Lordship's note, there fell six inches and half of rain in the months of June and July last. In Mount's-Bay Cornwall, according to my ombrometer there fell

	In:	Tenths	Parts		inches	tenths	parts
In June	—	—	2 — 6 —	$\frac{1}{2}$	} 6	9	$\frac{1}{2}$
In July	—	—	4 — 3 —	0			

So that the rain in this part of Cornwall exceeded that at Carlisle almost half an inch. I could wish the gentleman at Carlisle would continue his observations, adding thereto a journal of Farenheid's thermometer, and that we had another equally curious at Cathness, at least Aberdeen.

It is some amusement to compare the journal of the weather in one part with the accounts in the papers of storms, heats, and drought, and their contraries, in another.

On the 11th of August, there was at Bruffels a most dreadful storm of thunder, lightning, and hail; at Ludgvan only misty-rain and showers.

On the 19th of the same month, when one of the most violent hurricanes ever known scourged some

parts of Kent from the West and South-West, it was calm, hazy, and sun-shine, and the wind at North-East, in Mount's-Bay, in the morning; in the evening South-South-East.

On the 2d of this month of Oct. there was a most violent storm on the Eastern coasts of Britain, from Yarmouth to Edinburgh; wind from the North-East and East-North-East; many ships distressed, many wrecked. What is remarkable, at the same time a like violent storm blew in the Western channel, along the coast of Cumberland, Lancashire, and Wales, but the wind from the West. In Mount's-Bay the wind was somewhat stormy and showery in the morning, the wind at West half North; in the afternoon windy and showery and sun-shine, West half South: You see how different, nay opposite, the winds, even in their extremest violence, are on the Eastern and Western coasts, where they have nothing between them but a narrow ridge of land. The cause of this remarkable opposition, I should be glad to see well explained. It must certainly have lain in the middle between the two forces; and it might contribute somewhat to the discovery, to know whence, and to what degree, the wind blew on the mountains in Scotland, and as far South as Derbyshire, from Sunday morning to Monday noon: but these are particularities not to be expected till the age becomes more philosophical. I remain, my Lord,

Your most obliged

and obedient Servant,

W. Borlase.

IX. *An*

IX. *An Account of a Hernia of the Urinary Bladder including a Stone: By Mr. Percival Pott, Surgeon to St. Bartholomew's Hospital, and F. R. S.*

Read Feb. 16,
1764.

A Healthy boy, about six years old, was suddenly seized with a most acute pain, at the bottom of his belly; during the time the pain lasted he could not discharge a drop of urine, tho' he frequently endeavoured. After about an hour and half, he became perfectly easy on a sudden, and pissed very freely.

A few days after this, a small tumor, about the size of a large pea, was discovered, in the upper part of the spermatic process, just below the groin. As this tumor was perfectly indolent, and gave the child no kind of uneasiness, no notice was taken of it. By slow degrees it descended lower and lower; and as it descended it seemed to increase in size: the boy was observed to make water oftener than usual, but without pain or difficulty. He was looked at by two or three practitioners in the country, who, not knowing what to make of it, advised the letting it alone; at last, in the space of five years, it got to the lower part of the scrotum; and, after it was got thither, it was observed to increase in size much faster than it had done before. The boy was at a great distance from London, and his friends could ill bear the expence of going thither with him; so that another year passed away after the tumor was got into the last-mentioned situation. At last, when he was about
thirteen

thirteen years old, the swelling becoming troublesome, and the people in the country not caring to meddle with it, he was brought to London.

Two or three gentlemen of the profession, to whom he was showed, took it for a schirrhous testicle, and advised the extirpation of it; to which the child's friends would not consent.

When he was brought to me, I examined him very carefully, and was satisfied that the tumor, (which was now about as big as a middling chestnut) was not formed by the testicle: but, though I was clear that it was not formed by that gland, yet I could not find any testis on that side.

The swelling was still perfectly void of pain; had a stony, incompressible, hardness; was troublesome to the child when at play or using any brisk exercise, but never gave him any uneasiness when he sat, or stood still. It had all the appearance of being dependent from the spermatic process; but the process, tho' it had neither the look nor the feel of being diseased, was yet too large, and too full for a child of that age, and larger and fuller than that of the other side. The perfect equality and smoothness of the tumor, its extreme, incompressibility and its being perfectly free from pain, even when pressed with some force, were the circumstances which induced me to believe that it was not the testicle; but, tho' I was in my own mind satisfied of that, yet I cannot say that I was by any means clear what it was; and all that I could determine, was, that it certainly ought to be removed; as well on account of the trouble it now gave, and its manifest disposition to increase,

increase, as that I could not foresee any great hazard that was likely to attend its extirpation.

From the uncertainty in which I was concerning the true nature of the case, I determined to act very cautiously. I made an incision thro' the skin and cellular membrane, from the upper part of the scrotum quite down to the lower; by which I discovered a firm, strong, white membranous cyst, or bag, connected loosely with the skin by means of the dartos; I dissected all the anterior part of this cyst, quite clean; and found, that, as I traced it upward, it became narrower, and seemed to proceed from the groin: This determined me to try if I could not free the posterior part of it also. - In doing this I discovered the testicle, which was much compressed, flat, very small, and lay immediately behind the tumor.

The dissection of the testicle and of the spermatic chord from the bag, and from its neck (which I was obliged to do in order to preserve the testis) took up some time, and gave me some trouble; but, when I had finished it, I found that the cyst was dependent from, or continuous with, a membranous tube, or duct, of about the breadth of a large wheat straw, which seemed to pass out from the abdomen, thro' the opening in the oblique muscle, along with the spermatic vessels.

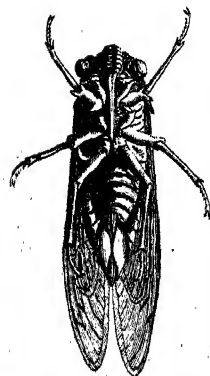
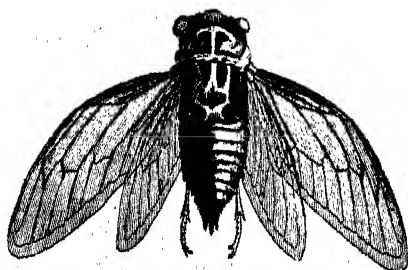
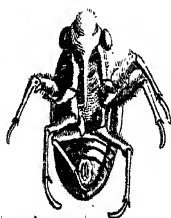
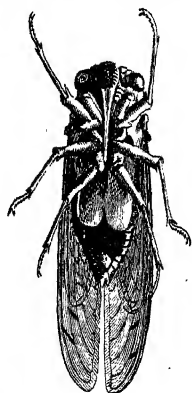
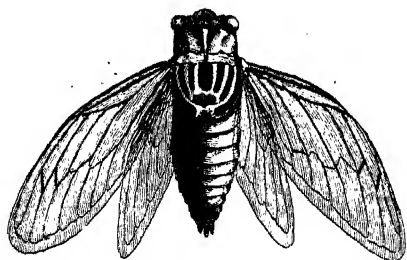
When I had perfectly freed this duct from all connexion, I cut it thro' immediately above the tumor; upon the division of it a quantity of limpid fluid (not less than two ounces) followed, and the mouth of the cyst expanding itself discovered a large stone, exactly resembling the calculi found in the urinary bladder;

bladder; which stone was closely embraced by the said cyst.

As there was not the least appearance of any fluid either in the bag or duct, before it was cut off, this discharge, together with the stone, induced me to suspect that the case was a Hernia cystica. In order to be certain, I staid some time; and, when I thought it was probable that some urine was derived into the bladder, I desired the boy to make water; he endeavoured so to do, and a full stream of urine flowed out through the wound in the groin, which put the case beyond all doubt.

I dressed him superficially; he had no bad symptom; his urine all passed out by his wound for a fortnight, or twenty days; at the end of which time, the wound gradually contracted; all the urine came through the urethra; and at the end of a month he was perfectly well.

P. Pott.



X. *Some Observations on the Cicada of North America, Collected by Mr. P. Collinson, F. R. S.*

London Nov. 2, 1763.

Read Feb. 23, 1764. **I**N Pennsylvania the Cicada is seen annually, but not in such numbers as to be remarkable; but at certain periods, of 14 or 15 years distance, they come forth in such great swarms, that the people have given them the name of *Locusts*. About the latter end of April these Cicadæ come near the surface: this is known, by the hogs rooting after them. They creep out of the ground, near the roots of trees, in such numbers, that in some places, the earth is so full of holes, it is like an honey-comb.

Their first appearance is an hexapode (an ill-shapen grub) with six feet. This is their middle or nymph state: they creep up every thing near them, and fix their claws fast, on the shrubs, and bark of trees: then the skin on its back bursts open, and the fly comes forth, disengaging itself by degrees, leaving the case or exuviae behind, in the exact shape, in which it was before occupied.

At first coming out, the Cicadæ are all white, with red eyes, and seem weak, and tender; but next day they attain to their full strength and perfection, being of a dark brown colour, with four finely-veined transparent wings, as will be better seen, than described, by the specimens before you. [TAB. VIII.]

They come forth out of the ground in the night ; being then secure from being disturbed by so many creatures, that prey on them, whilst they are under the operation of exchanging one state for another. From the tenth of May to the fifteenth, they are observed to be spread all over the country.

As soon as the dew is exhaled, the Cicadæ are very active, flying about from tree to tree. The male makes a singing noise, calling the female, which he effects by a tremulous motion he gives to two bladders, filled with air, under his wings. From their numbers the noise is so loud and troublesome, that it interrupts conversation with a continual din, from morning to evening. They continue coupling to the sixteenth of May: soon after the males disappear, and the females lay their eggs. They are much larger than the males.

They never could be perceived eating any thing; yet, as they are furnished with a long proboscis, which they frequently extend, they may suck the dews, or the farina of flowers.

The male, in coupling, hath, at the end of his tail, two hooks, with which he enters between the rings, that surround the body of the female. These, spreading internally, confine them long together; which may be requisite, as there is a great number of eggs to impregnate, some say six or seven hundred.

Soon after this work is over, the female begins laying her eggs. To assist her in this operation, she is armed with a dart near half an inch long, fixed between her breast and belly, and which extends to the end
of

of her tail. This she sheaths up, when it is not in use; with this dart she pierces the small twigs of trees, and, at the same time, injects an egg. The darted twiggs, that lie before you, will better shew the manner, than I can describe it.

It is surprising to see how quick they penetrate into hard wood, and croud it full of eggs, the length of two or three inches, ranged in a line close together, from twelve to eighteen in each partition. How she deposites the eggs in this direction, it was difficult to discover, they are so very shy whilst about this work: but my ingenious friend John Bartram, observing her, in the beginning of this operation, took a strong woody stalk of a plant, and, presenting it to her, she directly fell to work upon it, as he held it in his hand. It was very wonderful to see how dextreously she worked her dart into the stalk, at every puncture dropping an egg. This was seen very distinctly, as she did not touch the stalk with any other part of her body.

The Cicadae fix on most sort of trees, but like best the oak and chesnut, which are the twigs before you, and the sassafras, and all orchard trees.

They always dart to the pith of the branch, that, when the egg hatcheth, the little insect may find soft food in its infant state. When mature, they creep forth, go down the tree, or drop off, and soon make their way into the ground, where they have been found two feet deep. Here they find a secure repose, untill they have passed through their changes, from a magot to an hexapode, and lastly to a fly.

July 15th and 16th they were perceived coming forth: several darted twigs were perceived, and care-

fully examined, and opened: some eggs were hatched, others not mature, of a dull brown colour. These were taken out, and spread on a table; in about an hour the eggs cracked. It was very entertaining to observe, how the little insect contrived to disengage itself, from the shell. When it was got clear from its incumbrances, it run about, very briskly, seeking a repository in the earth.

Some General Remarks.

These Cicadæ are spread all over the country in a few days; but, being the prey of so many animals, their numbers soon decrease, and, their duration by the order of nature being short, quickly disappear.

They are the food of most kind of domestic and wild fowl, and many beasts: even the squirrels grow fat with feeding on them.

And one of the repasts of the Indians, after having first plucked off their wings, is to boil and eat them.

There are two distinct species of Cicadæ in North America; the one here described being much larger than the other.

The lesser species has a black body, with golden eyes, and remarkable yellow veined wings.

XI. *An Account of the Plague at Constantinople: In a Letter from Mordach Mackenzie, M. D. to Sir James Porter, His Majesty's Envoy Plenipotentiary at Bruffels, and F. R. S.*

S I R,

Read Feb. 23, 1764. **S**O many great men have written upon the Plague already, as Prosper Alpinus, Sydenham, Hodges, Diemberbroeck, Muratori, Mead, &c. that it might be justly thought presumptuous in me to touch upon that subject after them. But as I find, that they differ in some circumstances, and that some of them have had an opportunity of seeing only one year's plague; I may be allowed to write to you such remarks, as I have made for almost thirty years, that I have lived in this plaguy country, without any quotations or confirmations from other authors; which I hope will help to reconcile the different opinions of the above-mentioned famous authors. Which task I would choose rather, than to contradict them; for I am persuaded, that each of them wrote according to the best of his knowledge (as I do myself) without any intention of imposing in the least upon mankind.

It is beyond dispute, that the plague appears in a different manner in different countries; and that it appears differently in the same country in different years: for we find most other diseases alter more or less.

less, according to the constitution and disposition of the air in the same climate: for, some years, fevers are epidemic, and very mortal: other years, they are epidemic, but not mortal; the small pox the same; &c. And so the plague is some years more violent, and has some symptoms different from what it has in other years; which, I take for granted, must be the reason of any difference, that may appear in the remarks of the celebrated authors already mentioned. There is one extraordinary symptom, which the most of these authors mention, tho' none of them prove it, or pretend to have seen it; which seems to me inconsistent and incompatible with the animal œconomy; making still proper allowance for Omnipotence and Divine Vengeance, as in that of Sennacherib's numerous army, and many other such plagues, mentioned in Scripture. What I mean, is, that a person cannot die of the plague (such as it appears among us) instantaneously, or in a few hours, or even the same day, that he receives the infection. For, you know, Sir, by your long experience in this country, that all such, as have the plague, conceal it as long as they can, and walk about as long as possible. And I presume it must be the same in all countries, for the same reason, which is the fear of being abandoned and left alone; and so, when they struggle for many days against it, and at last tumble down in the street, and die suddenly, people imagine, that they were then only infected, and that they died instantly of the infection; tho' it may be supposed, according to the rules of the animal œconomy, that the noxious effluvia must have been for some time mixed with the blood, before they could produce a fever, and afterwards that corruption
and

and putrefaction in the blood and other fluids, as at last stops their circulation, and the patients die. This was the case of the Greek, who spoke with your master of horse, Knightkin, at the window, anno 1752, and went and died in an hour afterwards in the vineyards of Buiuk deré; and it was said he died suddenly, tho' it was very well known to many, that he had the plague upon him for many days before this accident happened.

Mrs. Chapouis found herself indisposed for many days, anno 1758, and complained pretty much, before she was suspected to have the plague. Captain Hills' sailor was infected in Candia 1736; was a fortnight in his passage to Smyrna, as the captain swore to me; yet he was five days in the hospital there before he died. Mr. Lisle's gardiner was indisposed twelve days before he took to his bed, and he lay in bed eight days before he died, in July 1745.

It is true, that Thucydides, in his account of the plague at Athens, relates, that some were said to die suddenly of it; which may have led others into the same way of thinking: but Thucydides (with all due regard to him) must be allowed to have known very little of the animal œconomy, for he was no physician, tho' a very famous historian; and he owns moreover, that, when the plague first attacked the Piræum, they were so much strangers to it at Athens, that they imagined the Lacedæmonians, who then besieged them, had poisoned their wells, and that such was the cause of their death. Besides, he pretends to affirm, from the little experience he had of the plague, that the same person cannot have it twice, which

which is absolutely false. The Greek Padré, who took care of the Greek-hospital at Smyrna for fifty years, assured me, that he had had the plague twelve different times in that interval; and it is very certain, that he died of it in 1736. Monsieur Brossard had it in the year 1745, when he returned from France; and it is very well known, that he and all his family died of it in April 1762. The Abbé, who takes care of the Frank-hospital at Pera, swore to me the other day, that he has had it already, here and at Smyrna, four different times. But, what is still more extraordinary, is, that a young woman, who had it in September last, with it's most pathognomonic symptoms, as buboes and carbuncles, after a fever, had it again on the 11th of April, and died of it some days ago, while there is not the least surmise of any accident in or about Constantinople since December, this only one excepted: but there died four persons in the same little house in September; and as the house was never well cleaned, and this young woman always lived in it, she was at last attacked a second time, and died.

The only antecedents, that I could observe to this malady, was a great murrain among the black cattle in May 1745, and in the beginning of June, the same year, swarms of butterflies flew about, and there were great numbers of caterpillars creeping every where, and afterwards a violent plague: and, after observing the same anno 1752 and 1758, you may recollect, that I foretold to you, Sir, that we should have a hot plague in those years; which accordingly happened, especially in the months of August and September 1758, when many of Marsellini's family,
Spathari,

Spathari, Skwackhiin's cook, Charlacci Rimbeault, Jackino's son, &c. died of it.

The plague is now more frequent in the Levant, than it was, when I came first into this country, about 30 years ago; for then, they were almost strangers to it in Aleppo and in Tripoli of Syria, and they had it but seldom at Smyrna; whereas now they have it frequently at Aleppo, and summer and winter in Smyrna, tho' never so violently in the winter; which must be owing to the great communication by commerce over all the Levant, and more extended into the country villages than it used to be. I take the plague to be an infection communicated by contact from one body to another; that is, to a sound body from an infected one, whose poisonous effluvia, subtilie miasmata, and volatile steams, enter the cutaneous sores of sound persons within their reach, or mix with the air, which they draw in respiration, and so advancing by the vasa inhalantia, mix with the blood and animal fluids, in which, by their noxious and active qualities, they increase their motion and velocity, and in some days produce a fever; so that the nearer and the more frequent the contact is, the greater is the danger, as the noxious particles, exhaling from the infected person, must be more numerous, and consequently have greater force and activity in proportion to their distance.

Some persons are of opinion, that the air must be infected, and that it is the principal cause of these plagues; whereas I presume, that the ambient air is not otherwise concerned, than as the vehicle, which conveys the venomous particles from one body into another, at least in such plagues, as I have seen hitherto

at Smyrna and Constantinople; allowing always, that the different constitution of the air contributes very much to propagate the plague: for the hot air dilates and renders more volatile and active the venemous steams, whereas cold air contracts and mortifies them. The person having the plague may be said to have a contagious and poisonous air in his room and about him, while at the same time the open air is free from any dangerous exhalations; so that I never was afraid to go into any large house, wherein a plaguy person lived, provided that he was confined to one room.

The pestilential fever shews itself first, by a chilliness and shiverings even in the months of July and August, so very like the first approaches of an ague, that it is impossible to distinguish the one from the other at first sight. This cold fit is soon accompanied with a loathing nausea and desire of vomiting, which obliges the patient at last to discharge a vast quantity of bilious matter, with great uneasiness and oppression in the thorax and mouth of the stomach, attended sometimes with a dry cough, as in an intermitting fever; and even in this stage it is very difficult to distinguish the one from the other. Next, the patient has a violent head-ach and giddiness, with some slight convulsive motions: he breathes hard; his breath and sweat stink; his eyes are ruddy, he looks frightened, sad, and pale; he has an insatiable thirst; his tongue is yellowish, with a red border; he has a total loss of appetite, restlessness, great inward heat, and more than could be expected from the fever, which is sometimes pretty moderate, but grows stronger frequently towards night: the patient very often bleeds

at the nose. He continues in that dismal condition for some days, untill the venemous matter begins to be separated in some measure from the blood, and discharge itself critically upon the surface by the cutaneous eruptions of buboes, carbuncles, blains, petechial spots, and some small vesicles or blisters: but all these symptoms are not to be looked for in the same person. When the cutaneous eruptions appear and grow sensibly, the patient finds himself better, and somewhat relieved from the great oppression he laboured under before. Some persons in the above state have a very violent fever, sometimes attended with a delirium and phrenzy; others are stupid, sleepy, and complain of nothing: as one of Captain Hill's men mentioned before; and the young fellow, who died of the plague last year, which he had in our palace; for I no sooner found, that he had a fever, and was at the same time so very stupid and senseless, but I concluded he had the plague, tho' it was strenuously maintained by the servants, that he had not been out of the kitchen for a month; but, upon strict examination, it was found, that he had many plaguy symptoms, as buboes, carbuncles, &c. upon his body, and that he had been in an infected house near the palace, about 12 days before; wherein no doubt he received the infection. Such as are furious and delirious, seldom live so long, as they who are sleepy and stupid; but if they live long enough to have the cutaneous eruptions push plentifully, and their phrenzy begins to abate afterwards, they may recover more probably than such as are sleepy and have a moderate fever; tho' I have known some of them likewise die; as Delaria, the French druggerman,

who went on horse-back on Friday to Giamderé, looking upon himself past danger, but died next Saturday morning. Marsellini's eldest son, 1758, thought himself so very well after the eruptions of the buboes, that he went from town, and dined at Therapea, and returned to town the same evening and died, after he had been delirious for some days before, and had had the plaguy fever from the time he left Buiukderé about ten days before.

I make no doubt, Sir, but you are very sensible, that nothing in this country, either air or diet, produces the plague, tho' both contribute very much to it's progress and violence, after it is brought here or to any part of this country from any other infected place; for you know, by long experience, that it rages most in the hot months of July, August, and September, when the diet of most of the poor inhabitants (who are the greatest sufferers by the plague) consists of unripe fruits, cucumbers, melons, gourds, grapes, &c.

The plague breaks out here and at Smyrna some years, when it is not possible to trace whence it is conveyed; for some houses, which were infected, and not well cleaned after the infected person is removed, lodge some of the venomous *moleculæ* in wool, cotton, hair, leather or skins, &c. all winter long; which, put in motion by the heat in April or May, breathe out of their *nidus*, where they resided, and recover so much life and action, as to enter into the cutaneous pores of any person, who comes within their reach, and so infect him; as it happened at the French palace, at Mr. Hubsch's and at Caraja's house, for two or three years running. But plagues of this kind
seldom

feldom spread, and are never so fatal, as such as come from abroad.

Many are of opinion, that the heat kills the plague, as they term it, which is owing to a foolish superstition among the Greeks, who pretend, that it must cease the 24th of June, being St. John's day, tho' they may observe the contrary happen every year; and the strongest plague, that was at Smyrna in my time, anno 1736, was hottest about that time, and continued with great violence till the latter end of September, when it began to abate; but was not entirely over till the 12th of November, when *Te Deum* was sung in the Capuchins convent.

This mistaken notion may be in some measure owing to a wrong sense put upon Prosper Alpinus, who allows that the plague at Cairo begins to cease in the months of June and July, when the strong Northerly winds (called *Embats* or *Etesian* winds) begin to blow, which make the country much cooler than in the months of May, April, and March, when the plague rages most; which he very justly imputes to the great suffocating heats and Southerly winds, which reign during those months in that country: and it is then, that the ships, which load rice, flax, and other goods and merchandise for Constantinople receive the infection, and carry it with them hither; and, upon these goods being delivered to persons in different parts of the city, the plague breaks out at once with great violence among the trading people of the Greeks, Armenians, and Jews; for I have observed, both here and at Smyrna, that the Turks are commonly the last of the four nations, who are infected; but when the plague gets once among them, they suffer

fer most by it, because they take the least care and precaution, and their families are much more numerous.

The plague, as well as all other epidemical diseases, has it's rise, progress, state, and declension, when it begins to lose it's virulence, and many of the sick recover. Some years it is felt sporadically all the winter; and we hear some accidents in the Phanar, among the Greeks, among the Jews, Turks, and Armenians; and even among the Franks; for you may remember, that Pera was not clean all the winter 1762. Some years it lodges in the villages upon the Bosphorus; but during the winter it is never of any great consequence.

As to the cure of this disease, some are for bleeding plentifully, as Leonardus Botallus and Doctor Dover, &c. But in this country, it is reckoned infallible death to open a vein, and therefore bleeding is never used: But I am of opinion that a medium between these two extremes might prove more to the purpose; for, as it is an inflammatory disease, bleeding and emetics might be of use in the beginning, as soon as the patient is taken with the fever, especially if the fever is very hot and attended with a delirium or any violent head-ach; but after there begins a separation of the morbid matter, which the strength of nature, and the agitation of the fever, drive upon the surface of the body in buboes or carbuncles, bleeding or purging must prove very prejudicial; but gentle vomits might be of service even then, as they might drive out those cutaneous eruptions more powerfully than nature could do it without any help. The vomits likewise might prevent the return of the morbid

morbific matter into the blood, which frequently happens, and the buboes, &c. disappear, and the patient infallibly dies in a very short time. As the pestilential fever has many remissions, I am of opinion, that the use of the bark in the remissions might be of great service; as it proved anno 1752, when the French ambassador's servant was saved at Buiukderé, by means of some bark and ipecacuana, which I sent with directions to Padre Joseppé; and he was the only person, that recovered of all the gang, who were then taken ill in our village.

The practice in the hospital is after this manner: when any person is suspected, they give him a large dose of brandy with a dram of Venice treacle; and afterwards they cover him very well that he may sweat: for the first three days, he eats nothing but vermicelli boil'd in water, with a little lemon juice. On the fourth day they give him rice and water; which diet they observe strictly till the 15th or 20th day, when they begin to allow him very thin chicken broth, commonly called brodo longo, and they give him from first to last nothing but warm water to drink.

They apply first to the buboes and parotides a cataplasm of mallows and hog's lard, to advance maturation; and, after they are ripe and open, they dress them with basilicon ointment.

They apply caimack and sugar to the carbuncles for some days to cool them; and when they begin to separate, they apply a digestive of Chio turpentine with the yolk of an egg. They apply nothing to the blains and petechial spots, which appear and disappear again upon any part of the body every three or four days.

All this time they give the sick no medicines, besides Venice treacle for the poor, and some doses of bezoar for such as can afford to pay for it; and they never can be persuaded to change their method; for when you gave them Doctor James's powder, they never tried what effect it might have.

I am of opinion that all antiphlogistics should be used before the eruptions; and all alexipharmics and antiseptics after them; more particularly camphire, and some doses of bark always in the remissions of the fever, and blisters ought to be of great use in the sleepy and stupid plague, for rousing the animal spirits, and for giving them some motion: but they are never used here; and, as they live by custom, it is impossible to prevail upon them to change it.

As to preservatives, I think the best is to remove from the infected persons and houses, and to keep at a proper distance for many days from them.

Some are of opinion, that fire preserves from the plague, and purges the air; from whom I beg leave to differ; for I have remarked here, that cooks and cooks mates, who are always near the fire, suffer more by the plague, than any other set of people in proportion to their number. Besides, the fire enlivens and gives energy to the poisonous effluvia lodged about them, which otherwise might die and disperse in the open air, if exposed sufficiently to it. Fire moreover opens the pores, relaxes the fibres; and, as the hot weather propagates the plague, fire should do the same more or less; and for the same reason I imagine, that all perfumes must be of very little service.

The next best preservative I take to be moderation, and a diet of such meats as are of easy digestion,

tion, of a rich balsamic quality, and capable of producing a rich and generous blood. It is likewise a great preservative to be under no apprehension, and to guard as much as possible against dismal thoughts and imaginations upon such occasions.

Thus, Sir, I have laid before you, in a few words, all that occurs to me upon the subject; and as most of the examples related happened in your own time, and are consistent with your memory and knowledge, I hope you can bear witness to the truth of the facts, if you think proper to present them to the Royal Society; and if not, you may dispose of them as you please, for I wrote them in obedience to your desire, and to give you an evident proof of the profound respect, with which I am,

Constantinople,
St. George's day,
1763.

Sir,

Your most obedient

humble servant,

Mordach Mackenzie.

- 1748. The plague began the 10th of May, and ended in November.
- 1749. It began the 16th of March, and ended the 20th of October.
- 1750. It began April 21st, and ended the 17th of September.
- 1751. It began the 15th of May, and continued all the summer, autumn, winter, and to the latter end of September 1752.
- 1753. It began May 31st, continued all the summer, autumn, winter, and till the 17th of September 1754.
- 1755. It began in June, but there was very little plague all this year.
- 1756. It began March the 6th, and ended the 12th of December.
- 1758. Then there was none till the 23d of April 1758, which ended in October.
- 1759. It began April 4th and ended about the 10th of September.
- 1760. It began April the 24th, and ended the 10th of November.
- 1761. It began the 10th of March, and continued till the 19th of December 1762. Since which day there has not been hitherto one accident, besides that of the young woman on the 11th of this month already mentioned.

In 1751, the 20th of October O.S. a vast quantity of snow fell, that cut off the distemper, and there was little plague in 1752. The former year was the most considerable, and more universally mortal at Constantinople than any in the space of fifteen years.

XII. *An Account of a remarkable Tide at Bristol : In a Letter to the Rev. Thomas Birch, D. D. Secret. R. S. from the Rev. Josiah Tucker, D. D. Dean of Gloucester.*

Read Feb. 23,
1764.

ON Saturday the 11th instant, when the tide had hardly begun to flow, according to it's regular course, it was observed, by the water-bailif of the city, and by several others, both on the back, and at the key, to rise very suddenly to almost high-water mark ; and it so continued for near half an hour : then it sunk, almost instantaneously, three feet perpendicular : after that, it began to flow in again, and kept flowing on till one of the clock, and rose to the height it was expected to do.

At Rownham Passage, a mile below the city, the ferry-men observed the tide to ebb almost instantaneously, and to sink at least four feet perpendicular. Then it flowed in again, as it should have regularly done.

At King-Road, which is about three miles below the city, the officers observed the king's boat to float suddenly, which they attributed to a great fresh coming. But they found afterwards the boat presently aground.

I could get no intelligence of any thing observable, that happened in the river Severn, excepting that at Gloucester, and at Worcester, the inundation sunk
very

very fast on that day. But most undoubtedly the strong rapid tide of the Severn must have been affected in a very remarkable manner, had there been any curious persons to take notice of it.

XIII. *A Letter containing some Experiments in Electricity, to Mr. Benjamin Wilson, F. R. S. from Mr. Torbern Bergman, of Upsal, in Sweden.*

Amplissime atque celeberrime domine,

Read Feb. 23,
1764.

EXperimentorum circa corporum adfrictum jam ea sunt commemoranda, quæ cum serico institui; cujus vis electrica, etsi a domino Gray dudum inventa, tamen hucusque parum fuit explorata. Pulchra sunt quæ adtulit dominus Symmer; sed non sufficientia. Ne vero longa narratione variorum tentaminum charta impleatur; palmaria tantum, et e quibus reliqua intelligi possunt, adferam. Nova ligamenta sericea, unum circiter pollicem geometricum Suecanum lata, adhibui. Alterum extremitatibus fixum et bene tensum, quod in posterum *fricatum* voco, alterum vero seu *fricans* manibus, quantum satis, expando, super eodem spatio fricati pluries duco reducoque perpendiculariter ad ejus longitudinem. Hanc operationem *transversam* adpello: eam vero qua idem spatium fricantis ducitur antrosum retrorsumque, super tota longitudine fricati, *secundum longitudinem* fieri dico.

Si

Si jam fricans et fricatum sint ejusdem telæ, adeoque textura, color, superficies, ceteraque, quam proxime paria, fricatio vero instituaturs transversa, fricans positivam et fricatum negativam contrahit electricitatem. In fricatione secundum longitudinem fricans fit negativum et fricatum positivum.

Si fricans sit alius coloris quam fricatum (modo non nigri) eventus est idem.

Si fricatum fuerit nigrum, hoc semper fit negativum, quocunque modo fricetur, excepto unico casu, quo nimirum fricans quoque nigrum. Tunc enim, secundum longitudinem fricatum, positive electrificatur.

Ex allatis vidi genus electricitatis solis circumstantiis frictionis determinari: ut vero modum invenirem, rem adcuratius pensitavi. In frictione transversa, fricatum, magis quam fricans, teritur. Nominetur attritus fricati et fricantis F, f respective, longitudo partis fricantis l , latitudo fricati a , numerus frictionum n , et erit semper $F = nf (l - a)$. Hinc duplex oritur effectus; nam et fricatum magis lævigatur, et magis calefit, quam fricans. Politura alia corpora disponit seu aptiora facit statui positivo; ideoque, heic, negativum ei adscribere nolui. Experimentum quoque feci, in quo fricans, multa frictione, erat lævigatum: sed nihilo minus, fricatum, rude, seu, nunquam antea adhibitum, inveniebatur positivum. Jure igitur in diversum caloris gradum cecidit suspicio: et hujus examinandæ ergo, varia tentavi; quibus tandem sequentia didici.

Si frictio instituiturs transversa, fricans vero antea probe calefit, hoc invenitur, operatione peracta, negativum, et fricatum, positivum. Tentamina hoc successu

cessu feci cum ligamentis, cœruleis, viridibus, luteis, rubris et albis.

Si fricatum fuerit nigrum, numquam positivam contraxit electricitatem, quamvis valde calefactum fricans : nisi hoc quoque nigrum.

Ex hisce itaque merito concluditur : *Calorem corpora* (saltem nonnulla) *statui negativo adaptare.*

Ubi de genere electricitatis quæstio est, nonne hinc varii errores, dum alterutrum ex confricandis, antea, fortioris vis ergo, calefit? Nonne hinc phænomena crySTALLI Islandica, frigore producenda?

Nexum, experientiæ congruenter, ita concipio. Quodvis corpus B (e. g. vitrum rasum) fieri potest vel positivum vel negativum; prout, vel $\tau\phi$ C (serico) vel $\tau\psi$ A (panniculo laneo) adfricatur. Est igitur fixus quidam ordo, quem corpora semper sequuntur, quamdiu circumstantiæ manent eadem. Sint A, B, C, D, E, corpora; quorum quodlibet, cum antecedentibus, sit negativum, cum sequentibus vero, positivum. Jam, quo minor inter confricata distantia, eo debilior, ceteris paribus, electricitas. Ideoque inter A et E fortior, quam inter A et B: si nimirum cetera paria. Hinc, inter corpora ejusdem generis, difficulter ulla, producitur vis; quæ, e contrario, cum distantia, crescit. Calor, uti probavi, statui negativo corpora adaptat; sed, si distantia memorata paullo major, eam quidem superare non valet; quod ex ligamentis nigris patet; interim tamen vim debilitat. Si globus electrificatorius incalescit, experientia quotidiana edocti, novimus, vim electricam debilitari. Nonne inde, quod calore magis adaptetur statui negativo, unde distantia fricans inter et fricatum minuitur? Sed de hisce, alia vice, plura; dum simul caloris effectus in alia corpora exponere liceat.

De existentia binarum electricitatum, memet, multa variaque luculentissima experimenta plene convicerunt, sed quid de natura electricitatis, sic dictæ, negativa, sit statuendum, adhuc dubius hæreo. Apices positivi *flant*, et negativi quoque; quod ad oculum monstrari potest; phosphoro illito, nam vapores alias vage e corpore non electrificato surgentes, instar caudæ cometæ, tam positivi quam negativi, ad magnam distantiam eiectiont. Difficulus cum privatione, rarefactione et adfluxu fluidi electrici conciliari possunt phænomena allata. Quæ tua sit mens, ut mihi proxime explices, oro.

Quæ in novissima epistola de metallorum electricitate scripsi; de confricatione, iisdem semper (quantum possibile) facta circumstantiis, intelligenda sunt: nam licet, initio, diu sola frictionis variatione, diversam, electricitatem procreare non possem, tandem vero, ad tuum præscriptum, feliciter operationem adgressus sum.

Instrumentum novum nuper invenit dominus Wilcke, quo, in campo, absque linea meridiana, acus magneticæ declinatio facile inveniri potest. Observationibus, hocce, Stockholmæ factis, æstate præterlapsa, acus 11 gr. 50 min. occidentem versus, declinare reperiatur. Hoc quoque utili invento, linea meridiana, absque negotio, ducitur.

Domini Mountine et Dodson, mappam, declinationes magneticas, pro anno 1756 (si probe meminerim) monstraturam: polliciti sunt, sed, num talis in lucem prodierit, adhuc ignoro.

Epistola, ad R. Societatem Upsalensem scripta, dudum lecta est.

Hocce autumnus, varii globi ignei visi sunt, meo teoro igneo, in parte prima voluminis LI. Transactio-
onum

onum descripto, valde fimiles. Fulmina quoque, ætate præterita, in meridionalibus præcipue regni partibus, grassata sunt. Ullusne, in Anglia, fulminis ictus, virgis ferreis æcclis, avertere conatus est? et quo successu? In Pensylvania tentari mihi narratum est. Certe si prudenter instituatur, nulla hinc mala metuenda video.

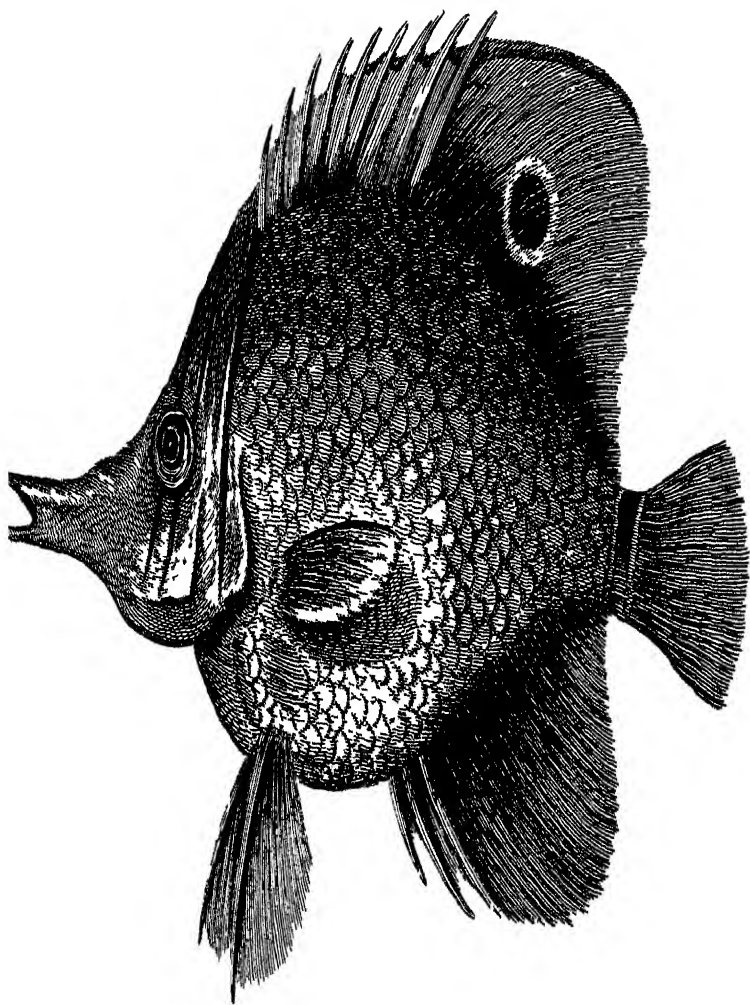
Omnigenam tibi optans felicitatem prosperitatemque, me tuo favori solitæque amicitiae commendo, et summo, quo par est, animi adfectu, permaneo,

Amplissimi atque celeberrimi Nominis tui

Cultor observantissimus

Dabam Upsalæ,
d. 18 Oct. 1763.

Torbern Bergman.



The relation of this uncommon action of this cunning fish raised the governor's curiosity ; though it came well attested, yet he was determined, if possible, to be convinced of the truth, by ocular demonstration.

. For that purpose, he ordered a large wide tun to be filled with sea-water ; then had some of these fish caught, and put into it, which was changed every other day. In a while, they seemed reconciled to their confinement ; then he determined to try the experiment.

A slender stick, with a fly pinned on at it's end, was placed in such a direction, on the side of the vessel, as the fish could strike it.

It was with inexpressible delight, that he daily saw these fish exercising their skill in shooting at the fly, with an amazing velocity, and never missed the mark.

In looking over that noble work of the Museum of the king of Sweden, printed anno 1754, I met with this *Jaculator*, well engraven, and described, by the learned Baron Linnæus, under the title of *Chætodon*, pag. 61, plate 33.

Baron Linnæus's Description.

Acanthopterygii-chætodon.

Chætodon rostratum, pinna dorsali postice macula fusca.

Corpus ovatum, compressum ; fascia grisea perpendicularis fecat caput per oculos. Fascia grisea perpendicularis ab initio pinnæ dorsalis descendit ante pectorales ad ventrales. Fascia grisea perpendicularis in

medio pisce. Fascia fusca saturatioꝛ cingit caudam ante radios. Macula fusca orbicularis in medio pinnæ dorsalis ubi mutica.

Caput rostro elongato, fere ut in syngnathis, dentes in maxillis minimi. Narium foramina utrinque 2 ante oculos, membranæ branchioſtegæ officula 5. Opercula branchiarum squamis tecta, ut in reliquis congeneribus. Pinnæ dorsi et ani æquales, valde transverſæ, et lateribus squamis tectæ. Dorsalis radiis 9-31 primoribus mucronatis, posterioribus 31 mollibus, longioribus. Pectorales radiis 14. Ventrals radiis 6 mollibus, excepto primo spinoso; eorum secundus reliquis longior. Ani radiis 3-20 posterioribus 20 longioribus, mollibus; primis 3 spinosis, caudæ radiis 14, æqualibus; parva

Accedit proxime ad LABRUM rostro reflexo fasciis lateralibus tribus fuscis. Amœn. Acad. 1. p. 313.

XIV. *An Account of the Polish Cochineal :
In a Letter to Mr. Henry Baker, F.R.S.
from Dr. Wolfe, of Warsaw.*

Warsaw, April 4, 1763.

Read March 29,
1764.

COCCI Polonici sunt ova, vel potius pupæ infecti nondum satis cogniti, quæ ad radices variarum plantarum adhærent, et versus finem Julii ab evulsis radicibus ope cultri abraduntur et colliguntur. Plantæ illæ sunt valde variæ, nec quotannis in una eademque specie reperiuntur

tur cocci illi, sed pro lubitu vagantur, hoc anno in hac, sequenti in alia planta. Communiter creditum, non inveniri nisi in sclerantho perenni calycibus fructus clausis, Linnæi, quod polygonum minus Bauh. folio et flore albicante, seminibus nudis oblongis. Hæc planta amat loca sabulosa; sed nimis est rara, ut notabilis cocci quantitas inde colligi possit. Uberior longe proventus est ejus in pratis pinguibus Podoliæ et Ucrainiæ: ibique invenitur supra omne genus fragariæ et potentillæ; sæpe etiam ad radices secalis, aliarumque plantarum, de quibus tamen nihil certi compertum habeo. Maxima copia collectum vidi ex potentilla alba Linnæi, fol. digitat. 5 natis, apice conniventi ferratis, caulibus filiformibus procumbentibus, receptaculis hirsutis: hanc nimirum indicare mihi videtur; ceterum ex fragaria flore albo, foliis lanceolatis, medio maximo, subtus villosis, supra viridibus cum tenui margine argenteo, caulibus debilibus hirsutis. Deinde ex pentaphyllo officinali, seu potentilla reptante Linnæi, fol. quinatis, caule repente, pedunculis unifloris. Postea etiam ex potentilla caulescente Linnæi fol. quinatis apice conniventi ferratis, caulibus multifloris erectis receptaculis hirsutis; de quibus specimina mitto.

Postquam copia horum cocculorum collecta est, immittuntur in ollam, et supra ignem torrentur quousque vermes enecati arbitrantur. In Augusto, insectum, ovum suum relinquit, et in planta tarde decurrit. Est insectum seminis cannubis magnitudine, totum molle, infra planum, supra ellipticum, seu ovatum, rugis transversis semicircularibus decem circiter a capite ad anum, quæ rugæ in inferiori abdominis parte in marginem quasi vel fimbriam coeunt, secundum circumferentiam abdominis inferiorem.

Caput

Caput parvulum; thorax supra vix conspicuus. Color totius animalis obscure purpureo-brunneus. Totum corpus pilis tenuibus, argenteis longis (respectu insecti) undique tomentosum, ut videatur pulverulentum, vel farina alba confersum. Pedes sex valde breves, minuti, nigrore splendentes, instructi unguibus acutis duobus. Antennæ duæ filiformes perbreves nigerimæ: rostrum reflexum perbreve. An abdomen pene fetosum? ut dicit Linnæus. Saltē pili ibi videntur paulo crassiores et longiores, sed similes reliquis. An volatilia fiant, expiscare nondum potui, nec sexum quidem internoscere potui. Sed dabitur, Deo dante, opportunior occasio, in hæc inquirendi. Transformationes difficulter observantur, cum insectum delicatulum a quavis injuria facile vitâ privetur, et illo tempore intra fissuras radicum abscondat se. Optimam figuram hujus insecti nuper dedit Ledermüller Norimbergensis in observationibus microscopis.

Color inde lanæ, gossipio, lino, conciliatur dilute carmesinus. Modus tingendi talis est. Coquunt coccum in ahenis cupreis, cum liquore, quem *kwas* (acidum) dicunt, et qui in Podolia, Russia, et Ucraina, pauperibus pro potu ordinario inservit. Parant vero hunc potum *kwas* ex farina secalina, quam infundunt aqua multa calida, et in loco tepido relinquunt, donec fermentatione acescat, et limpida fiat. Quantum quotidie de hoc liquore ebibunt, tantum addunt novæ aquæ cum manipulo farinæ. Breviori tempore idem fit, si fermentum acidum panis secalini pistorum: cum multa aqua diluatur, et in locum tepidum reponatur. Jam in hoc liquore coccum diu coquunt: Enascitur spuma et pinguedo valde multa, instar sebi alba, quam sollicite semper auferunt, usque dum talis jam nihil appareat. Erit liquor pulcre sanguineus.

neus. Jam, lanam puram albam in alio ahenō cum simili liquore *kwas*, et mediocri aluminis quantitate decoquunt, et salibus his bene imbutam exsiccant. Tandem lanam ita præparatam, in liquorem illum sanguineum immittunt, et per aliquot minuta coquunt: sic in momento omnis color lanæ adhæret, et liquorem instar aquæ limpidum relinquit. Lanam sic tinctam aqua frigida abluunt et exsiccant.

Rudis hæc tractatio docet, quantum ille color emendari posset, si in vase stanneo, cum sale ammoniaco et solutione stanni tractaretur. Narrarunt mihi collectores, si animalcula viva colligantur et enecentur, colorem inde obtineri multo elegantiores; cui facile crediderim, si præsertim eadem sollicitudine colligerentur, ac sit cum cocco Mexicano (cui de cetero nostrum insectum valde simile videtur), et loco tostionis, in aceto enecarentur. Multum Chocimi inquisivi in id, quo Turci purpureo colore lanam inficiunt: sed tinctura illa non nisi in Asia minori exercetur. Omnes tamen dicunt, tincturam hanc obtineri ex baccis, quæ ad radicem plantæ Armeniacæ, quam Romam appellant, crescunt. Forte hæc planta eadem cum potentilla alba, et forte etiam pulchritudo coloris non nisi ab artificio tinctoris pendet.

Quantitas cocci hujus ad externos exportati, ex Podolia, facile aliquot millia librarum quotannis excedit, et præterea multum domi consumitur. Maxima pars in Turciam abvehitur, magna etiam Breslaviam venit. Constat libra una 8-10 florenis Polonicis, (five 4-5 shillings) et unâ librâ fere 20 libræ lanæ tingi possunt.

*Further Account of the Polish Cochineal: In a Letter
from Dr. Wolfe to Mr. Henry Baker, F. R. S.*

Read April 12, 1764. **L**AST summer I amused myself with the Polish cochineal. It is unknown and neglected in this country. The several kinds of *Potentillas* are here very rare, and it was only upon the *polygonum minus*, or *scleranthus perennis* Linnæi, that I found the cochinille. I gathered about 300 of the coccusses, and put them with the plants and some sand in large pots. They are of different sizes. The insects creep out of their coccusses from the beginning of June till the middle of August: about fifty got out under my eyes. They are all exactly of the same shape: but some are three times smaller than others, according to their coccusses. The coccus is a thin round white skin. The insects are all hairy more or less; some are of a darker colour, some more crimson; some seem somewhat longer, others broader. But these differences seem to depend on their voluntary extension, and on their age, because they grow from day to day darker and more hairy. No mouth is to be seen, but a deep plait or furrow at the upper part of the breast. Two extremely small dark points seem to be the eyes. The two horns are thick, twisted like a screw, of the length of the breast; they end in an obtuse point. The two fore legs are twice the size of the four hinder legs, they have all sharp black incurved claws. The shape of the wrinkles and furrows may be seen in the drawing. It is impossible to find marks of the sex; and though they join sometimes their anusses, yet they do it so loosely,

loosely, that it cannot be accounted for a copulation. They seem to eat nothing at all. They creep about the plant a week or two, going often under ground, and getting up again. Then they make themselves a deep cylindrical hole in the sand down to the hard bottom of the pot, the end of which they cover with a fine white silk growing upon their bodies. There they lay their eggs and die. Others, who are disturbed in their work, grow weary and white, as if they were powdered all over with a white meal, which through a glass appears to be very fine white silky hairs, coming out over all the body. At last they lay them down upon their backs: the silky hairs grow very fast, to the length of one inch and a half, and the insect twists with its claws the hairs all round its body, so as to resemble a small heap of cotton; but the hairs are so tender, that a small wind will tear and destroy it. In this heap of cotton they lay their eggs, from fifty to an hundred, and then they die. Thus they remain till the middle of July. Afterwards, though they make their holes, or their cotton heaps, yet they die without laying eggs. The eggs are crimson, transparent, scarce visible, long, and round-pointed at both ends. In a week's time the young insects creep out: they are like their parents, but smooth, transparent, and crimson. I presented them every day fresh roots of the polygonum, but I cannot say they have eat any of them. In a week or two they disappear, going under ground. I preserve all these things. The insects seem now all dead, and so do the young ones, buried up in sand: but I hope next spring to see them alive, and to prosecute their farther change. I have killed about one hundred of
the

the insects in hot vinegar, as it is done in Mexico; and now I shall attempt to dye some woolly threads in the common way of the scarlet dyers. In the microscopical observations of Ledermuller at Nuremberg, you will find tolerable drawings belonging to this matter. In the beginning of August, I found an extremely small white fly, somewhat like to what is supposed to be the male insect. It is a third part of the size of what is represented by Ledermuller. It has a body like a gnat, snow-white, powdered below, but gray shining upon its back, six tender snow-white legs without claws, a thick bulky head, two very small prominent eyes, two hair-like horns, two wings, large enough in comparison to the body, snow-white below, and shining gray above. The belly to the tail is taper, and at the tail are three white hairs, very tender, and four or five times the length of the whole fly. But as this was the single one amongst three hundred, and totally unlike in every part to the other insects, I doubt very much of its being of this genus.

I hope next summer will teach me more; and, if I should be happy enough to bring the matter to any clearness, I shall put my observations into some order, and send them over with proper drawings belonging to it. But as there is no doubt but this insect will be found as well in England as in Poland, I thought it proper to give you the account of my observations as far as they go. Perhaps somebody of yours will think it worth their while to look the next month of June at the *potentilla*, *fragaria*, and *polygonum minus* roots, and will very likely find these same things.

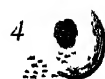
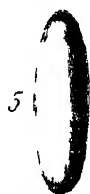
TAB. X. N°. 1. The cochineal insect of its natural size. 2. The same magnified. 3. The cotton. 4.

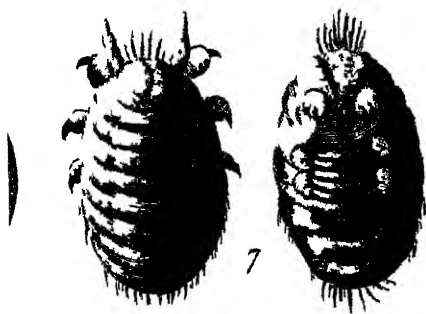
The cotton with the animal in the middle, and its eggs of the natural size. 5. An egg by the first magnifier. 6. Two coccuses greatly magnified. 7. The insect greatly magnified.

I send you also some of the insects killed in vinegar and dried. The cotton, and the supposed male insect. Some young insects. Some dead insects buried up in their cotton, some of which layed eggs, others not, some void coccus shells, some young ones, some eggs, etc. and also the polygonum minus.

P. S. The 12th of October, at 8 o'clock in the evening, we had here a strong aurora borealis. It lasted but a quarter of an hour. The shooting rays were white, and went all round from the horizon, making up at least three quarters of the circle of the horizon, the middle being just in the north. The rays pointed all towards one point of the heaven, which point was not the zenith, but at least 20 degrees farther directly against the South. It was a fair day. No wind or rain followed it; but the air was calm before and after.

Warsaw, Nov. 23, 1763.



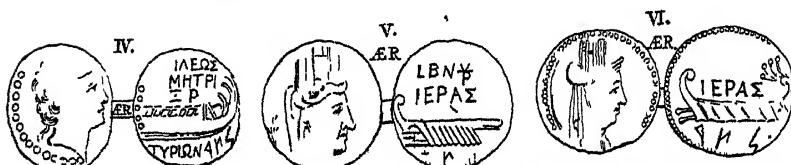




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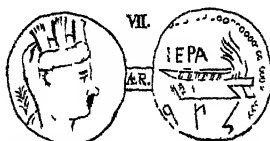
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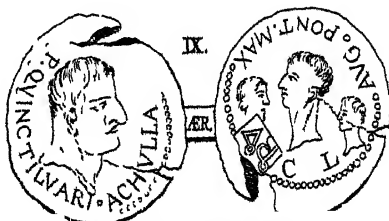
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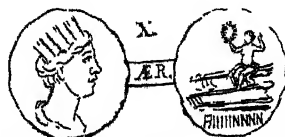
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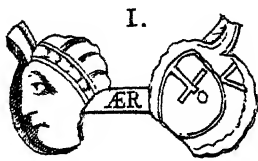
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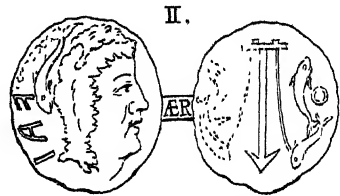
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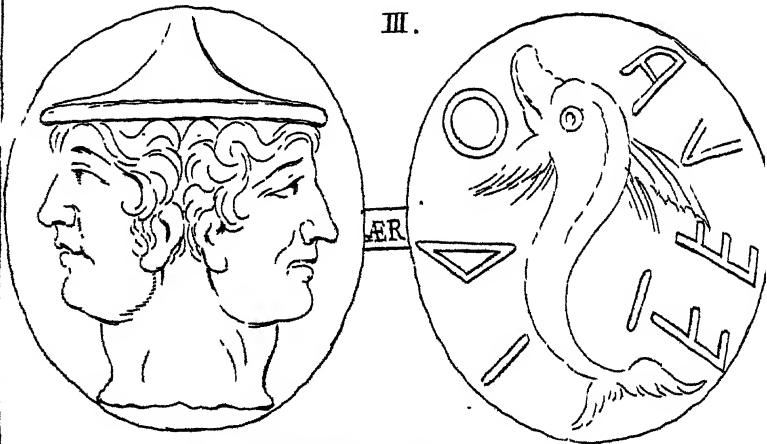
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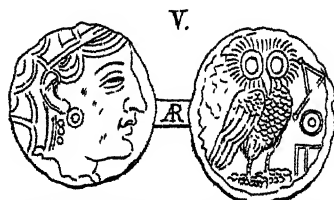
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Penes Joannem Swinton, STB. Oxoniens. R.S.S.

XVI. *Observations upon two antient Etruscan Coins, never before illustrated or explained. In a Letter to the Rev. Thomas Birch, D. D. Secret. R. S. from the Rev. John Swinton, B. D. F. R. S. Member of the Academy degli Apatisti at Florence, and of the Etruscan Academy of Cortona in Tuscany.*

Good Sir,

Read March 29, 1764. **T**WO small Etruscan coins, or weights, came lately into my possession, which I take to have been never hitherto published. Each of them is an uncia, or twelfth part of an as, and in pretty good conservation. The weight, or value, of each piece appears from a single globule on the reverse. As on several accounts they seem to merit the attention of the curious, and have not been yet explained; the Society will indulge me the liberty of imparting to them my sentiments of these valuable coins, and submitting to their consideration a few cursory remarks upon them here. [TAB. XI.]

I.

The first of these medals presents to our view a diademated head, greatly deformed by the injuries of
O 2 time.

time. The workmanship is rude, such as we find it to be in many of the more antient Etruscan coins. The slip of metal projecting from the round of the weight demonstrates the piece to have been cast, and may therefore be considered as a certain indication of it's high antiquity. From the globule and two letters, T V, on the reverse, we may infer this coin to have been a slips uncialis of the Tudertes, or people of Tuder, ΤΟΥΔΕΡ, as this antient city of Italy seems to have been called by (1) Strabo. It is at present known by the name of Todi. It went under the denomination of Tutere (2) amongst the Etruscans, and is pointed out to us by the letters T V, as here, on (3) several of their earlier coins.

That this piece is an Etruscan weight of very considerable antiquity, seems likewise evident from the barbarous and uncouth taste that appears in every part of it, in conjunction with the manner of writing, from the right hand to the left, visible on it, which antiently prevailed over all the (4) lucumonies of Etruria. The figure on the reverse, however rude and inelegant it may be, was perhaps intended to represent the prow of a ship, so frequently to be met with on this minute sort of coins. For a particular and satisfactory account of the early origin of

(1) Strab. *Geograph. Lib. x. Sil. Ital.*

(2) Anton. Francisc. *Gor. Mus. Etrusc.* p. 162, 427. Florentiæ, 1737.

(3) Anton. Francisc. *Gor. ubi sup.* p. 427. Honor. Arigon. *Num. Urb. et Populor. Etrur.* Tab. VII, VIII, VIII, &c. Tarvisii, 1745.

(4) Phil. Bonarot. *Ad Monument. Etrusc. Oper. Dempst. Addit. Explorat. & Conject.* § xlii. p. 90. Florentiæ, 1726.

such cast pieces (5) as this, recourse may be had to the very learned authors referred to here.

With regard to the diademated head on this piece, I shall only beg leave to observe, that a similar one occurs on an antient Etruscan coin published by (6) Arigoni, on a very old medal of Rome now in my possession (7), and undoubtedly on several other valuable pieces of a very remote antiquity, to be met with in the cabinets of the learned. This small Etruscan uncia weighs precisely three penny weights and one grain.

That the weight, or coin, I have been considering was current in Etruria long before the people of that country found themselves obliged to submit to the Roman yoke, will not, I think, admit of a dispute; but how many years it preceded that wherein the battle of the Lacus Vadimonis (8), so ruinous to the Etruscans, was fought, for want of sufficient light, both from history and the medal itself, I must not take upon me to decide.

II.

The second piece exhibits on one side the head of Hercules, adorned with a lion's skin; behind which a fish, resembling the turso, or tyrso, appears, attended by three Etruscan letters well enough preserved. On the reverse a dolphin, or tyrso, part of an anchor, and another fish, under the former, present themselves

(5) Idem ibid. § xxxviii. p. 78. Anton. Francisc. Gor. ubi sup. p. 421. *Una Lettera Al Signor Abate Barthelemy di Annibale degli Abati Olivieri*, p. 27. In Pesaro, 1757.

(6) Honor. Arigoni, ubi sup. Tab. XIV, XVI.

(7) See Plate Fig.

(8) T. Liv. *Historiar. ab Urbe Cond. Lib. ix.*

to our view. The other part of the anchor and the fish appertaining to it have been defaced, by the injuries of time; as we may collect from a similar *stips uncialis* formerly published by F. Montfaucon, then in the cabinet of the Marshal d'Estrées. A single globule, or uncial mark, determining the weight, or value, of the piece, is also visible here. The workmanship is somewhat rude, and different from that of the Romans. The tyrsó seems to allude to the origin and most antient name of the Etruscans, who were called Tyrsenians by the Greek writers that flourished before Polybius (10). That fish not seldom (11) occurs on the Etruscan coins. It is thought to have been one of the distinguishing sym-

(9) Montfauc. *Supplem. de l'Antiquit. Expliqu.* Tom. III. Liv. iv. c. 6. Pl. 48. p. 106. A Paris, 1724.

(10) Boch. *Chan.* Lib. I. c. xxxiii. p. 647, 648. Francofuti ad Moenum, 1681. Phil. Bonarot. *Ad Monument. Etrusc. Explicat. & Conject.* § xxxviii. p. 80. Florentiæ, 1726. Anton. Francisc. Gor. *Mus. Etrusc.* Vol. II. p. 421. Florentiæ, 1737.

(11) Phil. Bonarot. *ubi sup.* § xxxviii. Ant. Fran. Gor. *ubi sup.* p. 421. I have an exceeding fine Etruscan weight, or brass medal, of Volterra, adorned with the head of Janus on one side, done after the Etruscan manner; and on the reverse with a tyrsó, the obelus, or mark of the as, and seven Etruscan letters, forming the word VELITHERA, or VELITERA, the old Etruscan name of Volterra, one of the most antient cities of Tuscany. The piece is in the finest conservation, and weighs four ounces, fifteen penny weights, and eighteen grains.

It is farther remarkable for a figure of the A, which I never met with, before this weight fell into my hands, upon any of the Etruscan remains of antiquity. One of the sides of that element here is much shorter than the other, not unlike the form of the Greek *Alpha* exhibited by some of the earliest Athenian tetradrachms, particularly one in my possession, the most antient of any that I have hitherto seen.

bols of the Tyrſenian nation, with which the people of it were very well pleaſed.

The forms of the letters on this medal are exactly the ſame with thoſe of the correſpondent elements uſed in Umbria and the Proper Etruria, and apparently anſwer to the Roman letters FAI. They are ſufficiently clear and diſtinct, having ſuffered little from the injuries of time. The word they repreſent, in conformity to the Etruſcan (12) manner, points out to us ſome remarkable town. This may be certainly evinced from the inſcriptions preſerved on other Etruſcan coins. Now to what town can this word be ſuppoſed as a proper name to refer, but to Fæſulæ in the Proper Etruria? That city ſtood at the foot of the Apennines, and was one of the moſt (13) antient, as well as the moſt conſiderable, towns of Etruria. The Greeks wrote the name of it (14) ΦΑΙΣΟΤΛΑΙ and (15) ΦΑΙΣΟΛΑ, the earlier Latins (16) FAISVLAI,

Hence it ſhould ſeem to appear, that this valuable Etruſcan coin has never yet been publiſhed, at leaſt with a ſufficient degree of exactneſs. I therefore judged it would be by no means improper to ſend the Society a draught of it, as well as one of the Athenian tetradrachm here mentioned, taken with the utmoſt accuracy, and ſuch as may be entirely depended upon.

Vid. Annib. degl. Abat. Olivier. ubi ſup. p. 43.

(12) Anton. Franciſc. Gor. ubi ſup. p. 422-431. Joan. Baptiſt. Paſſer. Piſaurenſ. *De Num. Etruſc. Pæſtanor. Diſſertat.* Vid. *Symbol. Litterar.* Vol. II. p. 13-35. Florentiæ, 1748.

(13) Tho. Dempſt. *De Etrur. Regal.* Lib. iv. c. xix. & alibi, Florentiæ, 1724.

(14) Appian. Alexandrin. *De Bell. Ci. il.* Lib. II. p. 711. Amſtel. 1670. Dio, Lib. xxxvii.

(15) Polyb. *Hiſtor.* Lib. II. c. 25. p. 158. Amſtel. 1670.

(16) Enn. & Lucret. paſſim. Virg. Joh. Nicol. Func. Marburgenſ. *De Adoleſcent. Latin. Ling. Tractat.* p. 325. Marburgi Cattozum, 1723.

and those of the Augustan age *FAESVLAE*. The diphthong *AI* at first prevailed amongst the Latins, as well as the Greeks; but was afterwards converted by them into *Æ*, or *AE*.

That the Etruscans used globules on their coins, to denote the weight or value of them, has been observed by the most celebrated (17) antiquaries, and is now universally allowed. From them the custom of impressing these marks upon money and weights passed to the Romans. Now *Fæfulæ* was one of the most famous and antient cities of Etruria. Nay, with its district, according to a (18) very eminent writer, it formed one of the twelve *lucumonies*, or free states, of that country; and, in support of this sentiment, it may be remarked, that several valuable remains of (19) Etruscan antiquity have been found near the spot formerly occupied by that city. A mint was therefore undoubtedly erected there, and money coined in it. The situation of this place, at a small distance from the Arno, and not far from the Tyrrhenian Sea, may be considered as an additional reason why a *turso*, or *tyrso*, should have sometimes been impressed on its coins.

The age of the piece before me I cannot take upon me, with any precision, to determine; but think it must have been emitted from the mint at *Fæfulæ*, before the final subjugation of Etruria by the Romans.

(17) Phil. Bonarot. ubi sup. Honor. Arigon. *Num. Urb. & Pæfular. Etrur. Antiquissim.* Tarvisi, 1745.

(18) Dempst. ubi sup. et alibi. Vid. etiam Cluver. *Cellar. &c.*

(19) Anton. Francisc. Gor. ubi sup. p. 19, 112, 435.

We may therefore reasonably presume it to have appeared before the reduction of that country to the form of a province by the consul Ti. Coruncanius, in the year of Rome 473; if not before the terrible overthrow given the Etruscans by the consul Æmilius Barbula, in the year 442, which seems to have put a period to the independency of that nation. If we admit this, the coin cannot well be supposed to have preceded the birth of CHRIST less than three hundred years. Nay, it may be of a much earlier date, if the learned (20) Sig. Gori's notion of the high antiquity of the brass Etruscan coins be not altogether remote from truth. Be this as it will, no one has yet fully disproved or invalidated his opinion.

From the coin here considered, I think, we may venture to infer, that the manner of adorning with the head of Hercules some of the lesser weights was (21) originally Etruscan, but adopted afterwards by the Romans. The people of Fæsulæ and the neighbouring tract undoubtedly formed one of the twelve lucumonies, or cantons, of Etruria, and even made a figure after the Etruscan times. A colony was settled here by (22) Sylla, and the inhabitants of this place seem to have enjoyed the privileges of a municipium in the days of (23) Pliny. Other points, besides those already mentioned, are clearly deducible from the medal I

(20) Idem ibid. p. 419. Phil. Bonarot. ubi sup.

(21) Anton. Francisc. Gor. ubi sup. p. 424, 425. Phil. Bonarot. ubi sup.

(22) Cic. in *Catilin.* Orat. III.

(23) Plin. *Nat. Hist.* Lib. VII. c. XIII. p. 381. Ed. Hard. Parisiis, 1723.

have been endeavouring to explain, the only one of Fæfulæ hitherto discovered, which at present I cannot so much as touch upon; having but just time to assure you that I am, with the most perfect consideration and esteem,

S I R,

Your most obedient humble servant,

Christ-Church, Oxon.
May 31st, 1763.

John Swinton.

XVII. *Observation of the Eclipse of the Sun, the 1st of April 1764, made in Surry-street, in the Strand, London: By James Short, M. A. F. R. S.*

Read April 5, 1763. **T**HE morning of the eclipse I had provided the instruments I judged would be necessary for observing it in such a manner as to be satisfactory to the Royal Society as well as to myself. A reflecting telescope of two feet focal length, it's aperture four inches and a half, and it's magnifying power seventy times. To this telescope was fitted a micrometer with an achromic object-glass of 40 feet focal length.

The right honourable the Earl of Morton, now President of this Society, was pleased to honour me with his company, and also to observe; but in different rooms, out of sight and hearing of one another. His Lordship used a reflector of only eighteen inches focal length, four inches and a half aperture, and a power of forty times, to the eye-piece of which a helioscope was adapted, for viewing the Sun distinctly, without the least inconvenience to the eye.

The condition of the air was very unpromising, for, besides a general haziness of the sky, thin slow moving clouds were frequently passing over the Sun
from

from the South-west, so that it was by fits only that the Sun's limb could be seen distinctly. I used a smoked-glass to defend my eye, and my observations were noted down as follows.

Apparent time.

Apparent time.

	^h	'	"	{	the beginning of the eclipse by me. by Lord Morton.
March 31,	21	4	33		
		4	36		

All the rest by myself, with the before mentioned micrometer.

21	^h 12	ⁱ 27	—	^h 13	ⁱ 22, 0	= distance of the cusps.
	14	12	—	14	32, 1	= ditto.
	16	17	—	15	50, 8	= ditto.
	18	1	—	16	50, 6	= ditto.
	19	37	—	17	45, 4	= ditto.
	48	42	—	27	7, 1	= ditto.
22	19	15	—	29	33, 2	= ditto.
	22	28	—	29	49, 5	= { Moon's diameter nearly parallel to the horizon.
	23	58	—	29	49, 5	= ditto.
	26	10	—	2	58, 7	= { greatest distance of Sun and Moon's limbs.
	28	28	—	2	31, 3	= ditto.
	30	43	—	2	26, 2	= ditto.
	32	8	—	29	49, 5	= { Moon's diameter nearly parallel to the horizon.
23	35	23	—	21	11, 4	= distance of the cusps.
	37	33	—	20	18, 4	= ditto.
	40	59	—	18	52, 9	= ditto.

The end could not be seen for clouds, but the whole of the eclipse may be determined from the above measurements.

The Sun's diameter parallel to the horizon, about an hour before noon on the day of the eclipse, was $31' 59''$, 4, air hazy. The next day at the same hour it was $31' 58''$, 6.

A D D I T I O N.

April 12.	11	0	0	P. M.	Moon's diameter measured	=	33	49, 0	
13.	6	30	0	P. M.	=====	=	33	8, 8	
	10	25	0	P. M.	=====	=	33	20, 7	air undulating.
14.	6	30	0	P. M.	=====	=	33	21, 6	
	11	0	0	P. M.	=====	=	33	39, 5	air undulating.

XVIII. *Observation of the Eclipse of the Sun, April 1, 1764: In a Letter from Dr. John Bevis, to Joseph Salvador, Esq; F. R. S.*

S I R,

~~Recd April 5,~~
~~1764.~~ THE honour you were pleased to do me by sending me an invitation to observe the late eclipse of the Sun at your house, and the accommodations I there met with, require that I should give you the best account I can of my observation, However imperfect through the unfavourableness of the we

You may remember, Sir, that, not long before the beginning of the eclipse, the air, from very serene, turned hazy, and thin clouds came from the South-west. I had set a stop watch of Graham's, by a clock likewise of his, with which the Sun's transit on the meridian was observed carefully two days before the day of the eclipse, and the next day after it. By comparing my watch with this clock on my return, I found it was 19 seconds too fast in mean time, at your house, when I observed the beginning; and whereas it then shew'd $9^h 9' 12''$, it should have shewed no more than $9^h 8' 53''$, from whence subtracting $3' 45''$, the equation of time, there remains $9^h 5' 8''$, the apparent time of the beginning of the eclipse, as I observed it.

But I must remark, that, the state of the sky continuing such as I have described it, the beginning must have really happened sooner, by 10 or 15 seconds, as I judge from the first perceivable distance of the cusps; so that, if I state it at $9^h 4' 53''$, I presume I shall err but a very few seconds.

About the middle of the eclipse, the air was very clear, and the cusps well defined, which wanted about 60 degrees of joining. I could not then discern any thing on the Sun about the Moon's limb, which in the least indicated a lunar atmosphere. A full digit of the Sun, or more, remained uneclipsed. The day-light was but inconsiderably diminished, so that neither Jupiter nor Venus could be seen, though both in a favourable position, to the east of the Sun.

Fahrenheit's thermometer, placed without door to the north, stood at 50 when the eclipse began, and fell but one division whilst it lasted.

The

The end of the eclipse could not be observed for thick clouds, to the great disappointment of all who had the curiosity to give their attention to this phenomenon in or about London.

I am, with great regard,

S I R,

Your very obedient humble servant,

Clerkenwell-Close,
April 4, 1764.

J. Bevis.

*The Moon's Eclipse of March 17th, 1764, observed
in Surrey-street, in the Strand, London.*

Apparent time.

- | | | | |
|----|----|----|--|
| 10 | 32 | 0 | the penumbra just sensible to the naked eye. |
| | 39 | 0 | the beginning, viewed with an opera glass. |
| | 48 | 30 | the shadow touches <i>Mare Humorum</i> : This, and those that follow, with a 9 inch reflector. |
| | 53 | 35 | <i>Tycho</i> touched by the shadow. |
| | 54 | 41 | <i>Grimaldi</i> touched. |
| | 57 | 30 | <i>Grimaldi</i> covered. |
| 11 | 46 | 30 | the shadow touches the southern border of <i>Mare Tran-</i>
<i>quillitatis</i> . |
| | 0 | 49 | 36 ————— touches the southern border of <i>Mare Cri-</i>
<i>sum</i> . |
| 12 | 13 | 44 | <i>Grimaldi</i> begins to emerge. |
| | 17 | 36 | ————— is quite emerged. |
| | 56 | 23 | <i>Mare Crisum</i> nearly bisected by the shadow. |
| 13 | 0 | 30 | <i>Tycho</i> out of the shadow. |
| | 2 | 40 | <i>Mare Crisum</i> out of the shadow. |
| | 16 | 30 | the end of the eclipse, with an opera glass. |
| | 24 | 0 | the Moon clear of the penumbra. |

The shadow was ill defined, tho' the air was clear.

P. S. I find an observation of mine of the lunar eclipse of May 7th, 1762, printed in the Philosophical Transactions Vol. LII. pag. 543. How it got there I know not, never having thought it worthy the notice of the Royal Society. Besides, in the three observations there said to be made at Mr. Short's before the eclipse, the equation (I suppose by my own mistake) is *subtracted*, which should have been *added* to give the apparent time; and the three apparent times there set down, are to be increased each by $4' 16''$ to make them the true ones. J. B.

XIX. *Observations on the Eclipse of the Sun, April 1, 1764: In a Letter to the Rev. Thomas Birch, D. D. Secret. R. S. from Mr. James Ferguson, F. R. S.*

Reverend Sir, Liverpool, April 2; 1764.

Read April 5, 1764. **H**AVING been at this place ever since the beginning of March, and hoping that the sky would prove favourable (as to my great joy it did) for observing both the lunar eclipse of March 17th, and the solar eclipse of yesterday, I proposed to captain Hutchinson, at whose house I stay, to have a meridian line drawn on the leads on the top of his house, in order to adjust his clock for observing the times of these eclipses by: and we got Mr. Holden, who is master of a mathematical school here, to do it for us, by several observations of the altitude

altitude and azimuth of the Sun by day, and of the stars by night; and there were such exact agreements found by many repeated observations, that no doubt could remain of the meridian's being very well ascertained. The same gentleman, who is justly esteemed to be a very accurate observer, and an able calculator, finds the latitude of Liverpool to be $53^{\circ} 22'$; and its longitude is generally thought to be three degrees west of Greenwich, but he believes it to be somewhat less.

The clock being duly adjusted by our meridian line, at noon, and the time being found by observations of several stars in the evening of March 17, the apparent time of the beginning of the Moon's eclipse was observed to be at $10^h 27'$ p. m. and the end at $13^h 11'$.

On the next day, I calculated the time of the ecliptic conjunction of the Sun and Moon for April 1, by Meyer's tables, as we have them published by Mr. Maskelyne, and then made a projection of the Sun's eclipse for that time by them, for this place, according to it's latitude as determined by Mr. Holden, and supposing it's longitude to be 3 degrees west from Greenwich; and put up this projection in the council-room, that it might be seen, in order to find how it might agree with observation.

Being provided with a good reflecting telescope at captain Hutchinson's, I cut a round hole in a paste-board which would go tight on the tube, and took the Sun's image on a paper behind it, as large as I could have the image of the Sun sharp and well defined around the edge, which was included in a circle of 4 inches diameter. I divided the diameter into 12 equal parts, for digits, and each digit into 4 parts, the

the half of every fourth part being left to be estimated by the eye.

Mr. Holden and two other gentlemen, who are esteemed good observers, and were provided with refracting telescopes and Hadley's quadrants, were with me on Sunday morning, and I desired a third gentleman to note down the times, and to be careful not to mistake the minutes of time; as one might be more apt perhaps to mistake the minutes than the seconds. The clouds threatened us disappointment till about ten minutes before the calculated time of the beginning of the eclipse, and then the Sun shone out very clear; and during the time of observation we were but seldom interrupted by thin flying clouds. The first and last contacts of the Moon and Sun were so sharp and instantaneous, that it seemed possible to determine them within one second of time. Several altitudes of the Sun were taken during the eclipse, by reflecting the Sun's image from a basin of treacle; and the quantities eclipsed were plainly visible on the fore-mentioned image of the Sun on the paper, even to the eighth part of a digit. But the altitudes want yet to be corrected by their respective refractions.

Several people came into the room to see the eclipse, some of whom were subscribers to my lectures; and I could not well refuse them admittance. But I told them before-hand that they must neither speak nor move till the eclipse was found to be begun. This they strictly complied with, and gave no manner of disturbance; and, after it was begun, I desired them all separately to come and view it by the telescope, which hindered me from observing the number of digits eclipsed for the first hour.

I kept

[III]

I kept by the reflecting telescope, and watched the Sun carefully for about five minutes before the calculated time of the beginning of the eclipse. Our watches were adjusted to the mean or equal time; and two of them kept exactly alike during the whole time of the eclipse. The observations were as follows :

^h			Digits.	Sun's altitude.
8	59	0	eclipse begun	28° 37' 00".
10	2	0	—	8 $\frac{1}{2}$
10	5	0	—	9 — 35 49 30
10	11	0	—	9 $\frac{3}{4}$
10	13	0	—	10 — 36 29 0
10	18	0	—	10 $\frac{3}{8}$
10	21	0	—	10 $\frac{1}{2}$ — 37 0 0 uncertain..
10	25	0	—	10 $\frac{1}{2}$ — 37 20 0
10	30	0	—	10
10	38	0	—	9
10	40	30	{ Cusps perpendicular, by a plumb-line's shadow on the Sun's image. Sun's altitude then 38° 48' 0".	
10	43	30	—	8 $\frac{1}{2}$ — 38 57 30
10	47	0	—	8 — 39 8 30.
10	54	30	—	7
11	0	0	—	6 $\frac{1}{2}$
11	3	45	—	6
11	12	0	—	5,
11	15	45	—	4 $\frac{1}{2}$.
11	19	15	—	4.
11	28	0	—	3 { uncertain, on account of a thin flying cloud.
11	35	2	—	2
11	45	15	—	0 $\frac{1}{4}$ uncertain by another cloud.
11	50	45	{ eclipse ended; the sky quite clear. Sun's altitude 41° 27' 0".	

All wrote down by Mr. Baxtonden, who kept a copy thereof.

At night, Mr. Holden returned and examined the clock by the stars, and found the time shewn by the clock to be true.

Between the beginning and the middle of the eclipse, we could plainly perceive inequalities in the Moon's eastern limb on the Sun, by means of the reflecting telescope; and I often observed little tremulous bright specks of the Sun's lowermost edge in the otherwise dark place just before, or west, of the lowermost cusp; but they vanished in an instant, except one which was considerably larger than any of the rest, and was visible for about two seconds of time by estimation: but I was so intent upon observing it, and looking for others, that I forgot to have the time of its appearance marked down. This undoubtedly was owing to a dent or valley in that part of the limb of the Moon, which no hill beyond it took off from the sight. But as the eclipse was drawing toward the end, we could perceive no inequalities of the Moon's western limb on the Sun, nor any such specks in the Sun's edge about either of the cusps.

As the Moon's latitude was north ascending, and the cusps not perpendicular till after the middle of the eclipse, I apprehend that when they were so, the apparent altitudes of the centers of the Sun and Moon were equal. But whether they were then so or not, I leave to better judgements to determine.

I shall now set down the times of the beginning, middle, and ending of the eclipse, as pre-determined by the above-mentioned projection thereof for Liverpool,

verpool, from Meyer's tables, which were the apparent times; and shall reduce the observed equal times to the apparent, by subtracting 3 minutes 48 seconds (which we suppose here was the equation of time) from the equal times as observed by the clock and two watches which kept equally going together..

		Apparent times.	
		By projection. h	By observation. h
Beginning	— — —	8 56 0	8 55 12
Middle	— — —	10 21 45	not certain.
End	— — —	11 48 0	11 46 57
Duration	— — —	2 52 0	2 51 45
Digits eclipsed. — — —		$10\frac{2}{3}$	$10\frac{1}{2}$ exactly.

We wish for the accounts of the observed times at the Royal Observatory and at London; because, by comparing the difference, and making allowance for the velocity of the penumbra between Liverpool and London, the longitude of Liverpool might be known.

As the observed quantity was somewhat greater than the projected, as to the digits, and the projection which I gave in, some time ago, to the Royal Society from Meyer's Elements, made the lower edges of the Sun and Moon to be very nearly in contact at the time of greatest observation at Greenwich, I am apt to think that the appearance at Greenwich was annular: and am, with the greatest esteem,

Reverend Sir,

Your most obliged humble servant,

James Ferguson.

XX. *Observations on the Eclipse of the Sun,*
April 1, 1764, at Brompton-Park : By
Mr. Samuel Dunn.

Read April 5, 1764. **P**ART of the instruments which I was provided with for observing this eclipse were, a reflecting telescope 21 inches focus with a micrometer, a refracting telescope 6 feet in length with only two glasses, a double convex object glass, and a double convex eye glass. Two pendulum clocks, and a stop watch to seconds of time. I had taken as much care as possible to adjust the clocks to mean solar time some days before the eclipse, and from several altitudes of the Sun, taken March 30th in the morning and afternoon, concluded the clocks were then exactly with the Sun to a second of time.

March 31st was a cloudy day, and not the least hope of seeing the Sun the day following, 'till near midnight, when it cleared up, and the stars appeared.

April 1st, the Sun rose a little obscured by vapours and thin clouds, which he became more free from as he advanced in altitude.

At 6^h 56' per clock, I took the altitude of the Sun's centre $2^{\circ} 25' 20''$, from which observation the clock was 6 seconds of time before the Sun.

At 6^h 59' per clock, I took the altitude of the Sun's centre $12^{\circ} 50' 45''$, from which observation the clock was 4 seconds of time before the Sun.

At

At 7^h 5' per clock I took, the altitude of the Sun's centre 13° 49' 55'', from which observation the clock was 4 seconds of time before the Sun. And the same day in the afternoon,

At 4^h 23 per clock, I took the altitude of the Sun's centre 18° 49' 40'' from which observation the clock was 5 seconds of time before the Sun.

These and other observations confirmed that the clock was 5 seconds of time before the Sun at the beginning of the eclipse.

At 1^h 45' per clock, I set my watch exactly by the clocks; captain Bentincké and captain Holland were present with curious watches.

From 8^h 45' per clock, to the beginning of the eclipse, I continued observing with the reflecting telescope, and saw the limb of the Sun through the telescope and thin vapours, without any dark glass, as clearly as it could be seen by any helioscope whatsoever.

Captain Bentincké pronounced the seconds of time as they were fulfilled by the watch, and as the minutes of time were fulfilled they were written down.

At 9^h 4' 29'' per watch, I thought I saw a little dull tremulous vibration obtrude itself on the limb of the Sun; and,

At 9^h 4' 30'' it became a little more sensible; and,

At 9^h 4' 31'' a little more sensible; but it was

At 9^h 4' 32'' per watch before I was certain the Sun's limb was touched by the limb of the Moon; And,

At 9^h 4' 33 I plainly saw, through this telescope, and the thin vapours of the atmosphere, the least visible dent, perfectly well defined in the Sun's limb.

I instantly compared the watch with the clocks, and found the watch had lost 4 seconds of time of the clocks, a property which it had before had, when taken out of the pocket and exposed to the cold air.

The watch was therefore but one second of time before the Sun at the time of observation, for Brompton park, which is exactly one mile from Hyde-park-corner, in the way towards Kensington.

Clouds prevented the end of the eclipse from being observed; at $12^h 3'$ the Sun appeared, and the eclipse was ended, and from the observations which I made, have drawn a map of the phases and ending, which is herewith, TAB. XII.

London, April 4, 1764.

Samuel Dunn.

A Delineation of the ECL as Observed at Brompton -

A Part of it
not entered on

The circles concentric with
the Sun, shew the Digits
Eclipsed, at the place of
Observation.

according to the Order of the Signs.
(From the Side of the S. TR.)
for each Minute of a Degree.

Equator -
End of the Eclipse

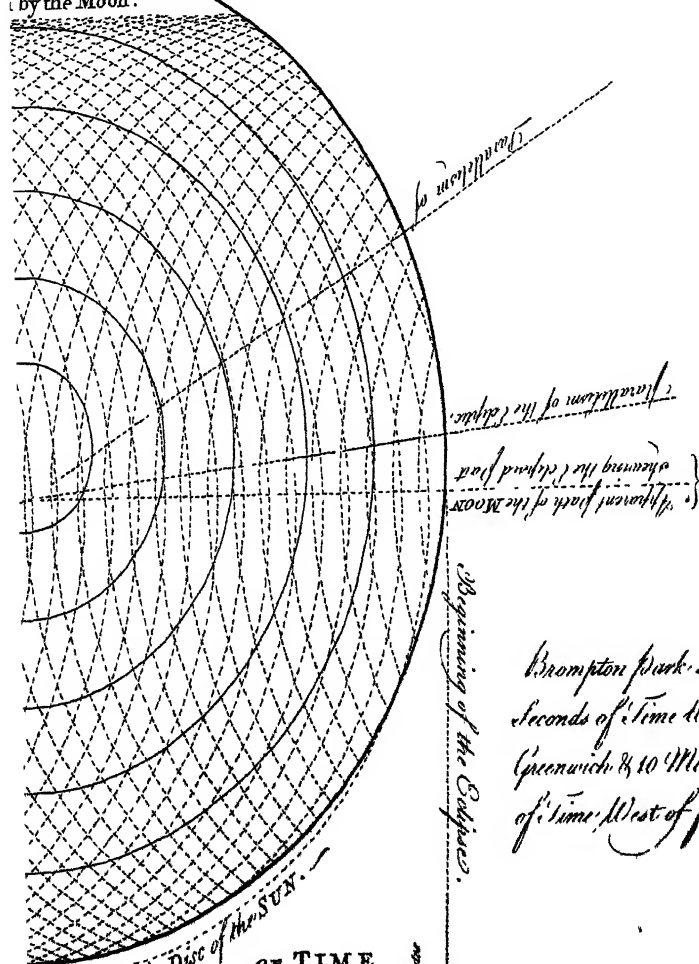


A SCALE of Digits Eclipsed.

Philos. Trans. Vol. LIV Tab. XII. p.
IPSE of the SUN. April 1. 1704;

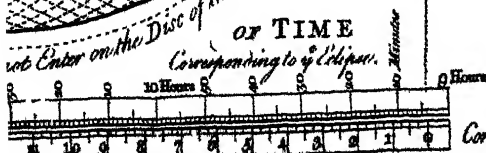
'Park', By Samuel Dunn.

by the Moon.



Brompton Park is 43 -
Seconds of Time West of
Greenwich & 10 Minutes -
of Time West of Paris.

OF TIME
Corresponding to $\frac{1}{2}$ Eclipse.



Corresponding to the Time &c.

Observations on the Eclipse of the Moon, 17th March 1764, made at Brompton-Park, near London, 10' of Time West of Paris, and 43' of Time West of the Royal Observatory at Greenwich: By Mr. Samuel Dunn.

Solar time.

- At 10 39 30 { the eclipse begins in that part of the Moon's limb
between Tycho and Grimaldus.
- 10 40 30 Skikardus toucheth the shade.
- 10 45 0 { the lower part of Mare Humorum is touched by
the shade.
- 10 51 10 Tycho and Grimaldus touch the shade.
- 10 53 0 { Grimaldus exactly covered and Tycho a little
immersed in the shade.
- 11 7 15 Galileus toucheth the shade.
- 11 11 45 Kepler and Lansbergius touch the shade.
- 11 18 10 { Copernicus toucheth the shade, and the lower
part of Mare Nectaris is a little immersed.
- 11 28 15 Ariadeus toucheth the shade, in Mare Tranquillitatis
- 11 36 30 Julius Cesar toucheth the shade.
- 11 41 0 { Manilius toucheth the shade, and Plinius not
yet immersed.
- 11 47 10 Mare Crisium it's lower part toucheth the shade.
- 12 15 40 Grimaldus is quite immersed.
- 12 17 45 Kepler emerges from the shade.
- 12 20 30 Copernicus emerges.
- 12 34 30 { the line of the shade passeth between Julius Cesar
and Manilius, and at the same time a little of
Mare Humorum is emerged.
- 12 46 25 { Mare Crisium begins to emerge; and at the same
time the line of the shade passeth a little below
Plinius and Vitruvius in Mare Tranquillitatis.
- 12 56 50 Tycho emerges.
- 12 57 20 { the centre of Tycho is a little emerged, and the
line of the shade passeth through Mare Crisium
two thirds from the upper end.
- 13 1 50 Mare Crisium is quite emerged.
- 13 5 45 Theophilus in Mare Nectaris emerges.
- 13 22 10 { the eclipse ends in that part of the Moon's limb
cut by a line drawn through the Moon's centre
and the lower end of Mare Nectaris.

Note, In the above observations, where it is said that any spot touched the shade, it is meant, that that spot was centrally bisected by the line of the shade at that time.

XXI. *An Account of the Degree of Cold observed in Bedfordshire: By John Howard, Esq; F. R. S. in a Letter to John Canton, M. A. F. R. S.*

S I R,

Read April 12, ^{1764.} **I** Would beg leave to acquaint you of a degree of cold that I observed at Cardington, in Bedfordshire, the 22d of November last: just before Sun rise Farenheit's scale by one of Bird's thermometers being so low as 10 and $\frac{1}{2}$. If it will throw any light on the locality of cold, or think it worth the Society's observation, would leave to your better judgment, and remain with great esteem,

S I R,

Your most obedient servant,

John Howard.

*The Phœnician Alphabet, deduced from two
Transcripts of the Maltese Phœnician Inscription.*

✱ ✱ ✱	Alph
9 9 9	Beth
	Ghimel
q q	Dalith
	He
γ	Vau
	Zain
⌘	Heth
	Teth
π	Jod
7 7 7	Capht
3 3 3	Lamed
4 4 4	Mem
5 5 5	Nun
μ μ μ	Samech
o o	Ajin or Ain
	Pe
ϕ	Tzade
9	Koph
q q	Resch
	Schin or Sin
h h	Thau

The Maltese-Phœnician Inscription.

9 a 3 4 ✱ q ϕ 3 o 9 5 9 9 3 4 3 3 3 a ✱ 3
 9 3 4 9 μ ✱ π ⌘ ✱ 4 a μ ✱ a 9 o ✱ 9 9 o
 o 4 4 7 a μ ✱ a 9 o 3 9 9 4 4 a μ ✱ 3 9 3 4
 4 7 a 9 π 4 3 9

The following paper was received a little before the recess of the Royal Society, in 1763; but was afterwards mislaid, and not found 'till the beginning of April 1764. To this accident is solely to be imputed the delay of it's publication.

XXII. *Some Remarks upon the first Part of M. l'Abbé Barthelemy's Memoir on the Phœnician Letters, relative to a Phœnician Inscription in the Island of Malta. In a Letter to the Rev. Thomas Birch, D. D. Secret. R. S. from the Rev. John Swinton, B. D. F. R. S. Member of the Academy degli Apatisti at Florence, and of the Etruscan Academy of Cortona in Tuscany.*

Good Sir,

Read April 12, 1764. **M.** l'Abbé Barthelemy having lately communicated to the learned world (1) a copy of one of the Phœnician inscriptions long since discovered in the island of Malta, more accurately taken (as he pretends) than any of those that had ever before appeared, and attempted to ex-

(1) *Mem. dans lequel on prouve, que les Chinois sont une colonie Egyptienne*, par M. de Guignes, &c. p. 39—54. A Paris, 1760.

plain it in a manner perfectly new; I shall beg leave to make a few cursory remarks upon what he has been pleased to advance, on this occasion. Which remarks may perhaps be deemed not altogether unnecessary, as part of the inscription, according to M. l'Abbé's lection of it, seems at least somewhat involved, if not wholly unintelligible; and consequently will admit, unless I am greatly deceived, of a farther illustration. Nor can M. l'Abbé be disgusted at my presuming to differ in a few particulars from him, as he is not acted by a spirit of ostentation, or a thirst after applause, but the love of truth; and as he has taken the same liberty with one of my dissertations, which the Royal Society did me the honour to publish a few years since, upon a similar subject.

I.

M. l'Abbé observes, that “ in (2) the beginning; “ the Phœnician letters were not distinguished from. “ the Samaritan; but that most of them in process. “ of time admitted of such great variations, that the “ traces of their origin are very frequently lost.” Hence it seems to appear, that the later any Phœnician inscriptions are, the more the forms of their letters must recede from those of the correspondent Samaritan; and, vice versâ, that the more remote the ducts of any Phœnician elements from those of the correspondent Samaritan are, the later such characters, and consequently the inscriptions formed of them, must be. Let this be allowed, and it will be sufficiently manifest, that the Maltese inscription (which

(2) Ibid. p. 39.

our author seems to have had principally in view, when he made the preceding observations) is several years at least posterior to the days of Simon, prince and high-priest of the Jews. The alphabet therefore deduced from this inscription, which differs pretty considerably from that exhibited by the sepulchral stones found in the ruins of Citium, ought not to pass for the true antient Phœnician alphabet, that prevailed over so great a part of the East in the earlier ages.

From the passage here produced we may farther infer, that the Phœnician inscriptions either coeval with the Samaritan coins struck by Simon, prince and high priest of the Jews, or older than those coins, must be formed of letters, for the most part, extremely similar to the Samaritan. And this we find in fact to be true. Many of the elements therefore of those inscriptions may be more easily discovered by the assistance of Simon's medals, than by that of any monument of antiquity several hundred years later. Nay, the powers of many letters, allowed to belong to the Phœnician alphabet, have been actually ascertained by means of the correspondent elements on the Samaritan coins. We must not therefore too hastily admit, or too closely adhere to, what M. l'Abbé has been (3) pleased to lay down, in the most unlimited terms, as a certain and indubitable truth; viz. that "a Phœnician alphabet ought by no means to be founded upon the affinity of its letters with those of other alphabets." The alphabets he himself has given us, incomplete as they are, will be considered as a sufficient refutation of this assertion.

(3) Ibid. & Planch. I.

What has been here observed of the Maltese inscription is, with regard to it's antiquity, at least, equally applicable to that of Carpentras (4), the ducts of whose letters are still more remote from those of the Samaritan; and consequently this, according to M. l'Abbé's decision, must be still of a later date. And, indeed, the rude and almost barbarous forms of it's Phœnician elements render this incontestably clear. Some of them are extremely similar to, if not apparently the same with, those of the correspondent letters on certain Spanish or African Phœnician coins, struck, as there is reason to believe, after the commencement of the Roman empire. And if this be the case, how can we suppose them to have been some of the first alphabetic characters that ever appeared, or those immediately deduced (5) from hieroglyphics themselves? They seem to have been only corruptions of the earlier Phœnician letters, from whose forms several of them have very considerably varied. So high an antiquity as that above supposed is not announced by the face of the inscription, and therefore the learned will not perhaps readily assent to such a supposition.

II.

The eleventh letter of the first line, which is taken for *Thau*, seems to have been a little mutilated by the injuries of Time; as part of the strait line cutting

(4) Ibid. p. 53.

(5) *Recueil d'Antiquit. Egypt. Etrusq. Grec. & Romain.* par M. le Comte de Caylus, Tom. I. p. 65. Plan. xxvi. A Paris, 1752.

the perpendicular, in this element, is effaced. That this is fact, appears from two tetradrachms of Menæ, an antient town of Sicily, at present going under the denomination of Menéo, now in my hands; on one of which the whole figure of this kind of *Thau* is perfectly well preserved, and on the other a similar figure is visible, though the original transverse line has been somewhat diminished. One of (6) Lord Pembroke's medals of the same town also presents to our view a *Thau* completely formed. We meet with this letter in the Citiean inscriptions, sometimes as it has been handed down to us by the Punic Medals of Sicily, and sometimes as it is represented in M. l'Abbé's plate of the Maltese inscription; part of it perhaps having been lost, in the course of so many ages. However, that the earlier Phœnician *Thau* frequently bore some sort of resemblance to the character taken for the same element on the Sicilian coins, there is great reason to believe; since otherwise it could not have resembled a cross, as it most certainly did. For that the antient Samaritan *Thau*, nearly agreeing in figure with the Phœnician, had the appearance of a cross, we learn from some good (7) authors. In the later periods, and perhaps to the time when the Phœnician alphabet itself began to be diffused, it might in certain countries, pretty remote from Phœnicia, have assumed a somewhat different form, though this I must not pretend absolutely to affirm; but that the inscription I am considering exhibited at

(6) *Num. Ant. &c.* à Thom. Pemb. et Mont. Gomer. *Cm. Coll.* P. 2. T. 87. Lond. 1746.

(7) Tertullian. Hieronym. &c. Vid. Val. Ern. Loescher. *De Caus. Ling. Eb.* p. 234. Francofurti & Lipsiæ, 1706.

first the complete figure of *Thau*, (8) from P. Lupi's draught of it, taken upon the spot, in 1735, I think we may fairly presume. And that the *Thau*, as it there appears, was not seldom used in the earlier Carthaginian times, from the medals of Menæ already touched upon, to omit others that might be produced, is, I conceive, incontestably clear.

III.

I cannot forbear suspecting, that M. l'Abbé has a little deviated from the genuine form of the *Aleph* in his plate; notwithstanding the accuracy with which, as he informs us, Count Caylus's copy was taken. That Phœnician element occurs upon my Punic and Phœnician coins, not to mention those of my friends, above thirty times; and yet not one of these characters exhibits an angle, formed of two right lines cutting the perpendicular, as does the *Aleph* here. Nor do we meet with such a figure of *Aleph* in the Citiean inscription, preserved on the original stone, brought from Cyprus by Dr. Porter, and presented to the University of Oxford by Charles Gray, Esq; member of Parliament for Colchester, and fellow of the Royal Society; though the usual form of this letter is found, oftener than once, in that inscription. P. Lupi's copy (9) of the Maltese inscription exhibits the *Aleph* (whose ducts were perhaps better preserved when that transcript was taken than at the time Count

(8) Sig. Ant. Fran. Gor. *Difef. dell' Alphab. Etrusc.* p. 102. & Tab. III. p. 109. In Firenze, 1749. Lup. *Letter. Philolog.* Let. XI. p. 6.

(9) Gor. *Difef. dell' Alphab. Etrusc.* & Lup. ubi sup.

Caylus's copy was sent him) as it appears on my Punic and Phœnician coins.

IV.

The eighteenth letter of the first line of this inscription is not *He*, as M. l'Abbé supposes, but *Mem*. This is rendered indisputable by the form of the element itself, as well as by the tenor of the inscription. That the form of the element perfectly resembles, or rather is altogether the same with, that of *Mem*, is sufficiently evident from M. l'Abbé's own plate, and even more so from the copy communicated to Sig. Gori by P. Lupi. And with regard to the tenor of the inscription, I shall not scruple to affirm, that this absolutely requires the letter to be *Mem*, and not *He*. For otherwise the word צִרָא, TZORA, or TZVRA, must denote Tyre; whereas the name of that city in Syriac, supposed by M. l'Abbé to be the language of the inscription, as well as Chaldee, Hebrew, Samaritan, and Arabic, is צִר, TZOR, or TZVR. To which we may add, that the four last elements of this line cannot form the word הוֹרָה, HOC VOTVM, THIS VOW, as M. l'Abbé asserts, because that would be neither Syriac nor sense; as the monument could not with any manner of propriety be termed a vow, and consequently could not be transmitted down to future ages under the denomination of THIS VOW. It was not a vow, but erected in pursuance of a vow. The eighteenth and the three preceding letters then form the words צִרָא, TYRVS MATER, or rather here TYRI MATRIS, OF TYRE THE METROPOLIS, as we find that
city

city actually styled on several Tyrian coins. This sense is perfectly consonant to the tenor of the inscription, and will therefore, I flatter myself, be readily admitted by (10) M. l'Abbé.

But to put the point here insisted on even beyond the possibility of a doubt, I shall beg leave farther to observe, that I have two of those Phœnician coins (11) attributed by M. l'Abbé to the city of Marathus; one of which exhibits the first letter *Mem* as it stands in his alphabet, (*М*) and the other exactly as he has presented to our view the pretended form (*У*) of *He*. Such demonstration as this must convince every one, that is not resolved to be proof against conviction, and be allowed absolutely decisive in this affair.

V.

Though M. l'Abbé has not inserted the last letter of the word עֲבָדוּ, in the beginning of the second line, that element was nevertheless most certainly *Vau*. This is sufficiently manifest from P. Lupi's (12) copy, as well as from the vacant space after *Daleth*, which is capable of containing only a single letter, and that can be no other than *Vau*. Besides, בִּרְכֵם, BENEDICAT ILLIS, not NOBIS, MAY HE BLESS, or PROSPER, THEM, not US, the last word of the inscription, sets this point in the clearest light. The next word but one, וְאָחִי, has been rendered by M. l'Abbé Barthelemy ET FRATER MEVS, though ET FRATER EIVS must be allowed much more con-

(10) *Journal des Sçavans*, Aout 1760. p. 267-271.

(11) *Ibid.* p. 275.

(12) *Gor. & Lup. ubi sup.*

sonant to the tenor of the inscription. I am therefore inclined to believe, that the word **ואחי** ought to be considered as in regimen, or construction; and that the proper name **עבדאסר**, ABDASAR, is understood after the last mentioned term. Instances of such an ellipsis as this occur, in PSAL. lxxiv. 19. ISAI. xiv. 6. &c. and have been produced by Buxtorf, in the piece (13) referred to here. To which we may add, that the person of the verb **עבדו** strongly countenances, at least, if it does not give an absolute sanction to, what has been here proposed to the consideration of the learned.

VI.

The proper name **עבדאסר**, in the second line of the inscription, was pronounced, as I apprehend, by the orientals ABDASAR, or ABDESER, not ABDASSAR. This is rendered not a little probable by the proper names ASAR-HADDON and TIGLATH-PIL-ESER, the *Samech* neither in ASAR nor ESER there having received a dagefeh from the Masorites. The same (14) point is also confirmed by the Septuagint. It likewise appears from the Etruscan **ÆSAR**, which is evidently the same word with the Phœnician and Chaldee ASAR, or ESER, and in common with it denotes GOD. That ASAR, or ESER, in general signified GOD, is allowed by a (15) learned writer;

(13) Johan. Buxtorf. *Thesaur. Grammat. Ling. Sanct. Hebr.* p. 363. Bafilæ, 1663.

(14) 2 KING. xv. 29. xix. 37. ISAI. xxxvii. 38. EZR. iv. 2. Vid. etiam Matth. Hiller. *Onomast. Sacr.* p. 607, 608.

(15) Matth. Hiller. *Onomast. Sacr.* p. 596. Tübingæ, 1706.
though

though that it was also applied to one particular deity, the words ASAR-HADDON, TIGLATH-PIL-ESER, &c. seem clearly to evince. The Gallic HESVS, taken by some for the god Mars, of a similar sound, may perhaps bring an accession of strength to what has been here advanced. The name ASAR, or ESER, therefore amongst the Phœnicians was probably both equivalent to the general term GOD, and likewise pointed out to them one particular deity. In ABD-ASAR, or ABD-ESER, it must have answered to BACCHUS, or DIONYSVS; the whole name corresponding with ΔΙΟΝΥΣΙΟΣ, BACCHICVS, OF OR BELONGING TO BACCHVS, THE SERVANT OF BACCHVS, the true import of the two words ABD-ASAR, or ABD-ESER, of which this Phœnician proper name is composed.

VII.

With regard to the next proper name, אֲסֶרִימֶר, אֲסֶרִימֶר, or rather אֲסֶרִימֶר, ASERIM-HAMMAR, M. l'Abbé and I differ considerably in our notions. He takes the fourth letter for *He*, and I for *Mem*. Now this character is exactly the same with that in the first line of the inscription, which I have already proved to be *Mem*, and consequently it must be considered as altogether the same element. Admit this, and the two words forming the proper name, אֲסֶרִימֶר, or rather אֲסֶרִימֶר, will immediately occur. I say אֲסֶרִימֶר, or rather אֲסֶרִימֶר, because such an ellipsis of the prefix (particularly (16) in the proper name עֲנִים־מֶלֶךְ, or עֲנִים־הַמֶּלֶךְ) may sometimes be

(16) Id. *ibid.* p. 603, 608.

observed. In fine **אסרם-המר**, or **אסרם-מר**, must be composed of **אסרם**, ASERIM, (the Phœnician name of one of the kings of Tyre, (17) according to Menander Ephesius) and **המר**, HAMMAR, IPSE DOMINVS; the term **מר**, MAR, entering into the composition of certain similar (18) names. Now ASERIM-HAMMAR being equivalent in Phœnician to the Greek, or rather Egyptian, SARAPION; the deity denominated SARAPI, or SARAPIS, in Egypt, must have assumed the name of ASERIM at Tyre, as from the inscription now before me may be very fairly inferred.

VIII.

That the first letter of the third line is *Mem*, and not *He*, as M. l'Abbé is pleased to affirm, both the form itself and the sense of this part of the inscription seem evidently to prove. The figure here is altogether the same with that of the eighteenth element in the preceding line, and consequently must be allowed to represent *Mem*. The sense also evinces this, beyond the possibility of a doubt. For, I believe, it will be no easy matter to prove the reality of M. l'Abbé's word **הנבן**, or at least to render it intelligible here. But with respect to the word **מן**, which I take the two first letters to form, equivalent in Syriac to the (19) Latin **QVI** (est), or **IS QVI** (est), it

(17) Menand. Ephes. apud Joseph. *Cont. Apian.* Lib. I.

(18) Matth. Hiller, ubi sup. p. 602, 603.

(19) Buxtorf. *Lex. Chald. & Syriac.* p. 301, 302.

clearly continues the sense, and is of course most perfectly consonant to the tenor of the inscription.

IX.

What has been observed of the last mentioned character is equally applicable to the eighth and twentieth elements of the same line. They are both indubitably *Mem.* This, not only by the powers of the letters with which they are immediately connected, but likewise by what has been already advanced, is rendered indisputably clear. 'Tis worthy observation, that M. l'Abbé Fourmont was the first (20) who took this character for *He*; and pretended, absurdly enough, to deduce it from the Estrangelo form of that element.

X.

With regard to the last word but one of the inscription, **במטעקלם**, or **במטעקלם**, it seems to be composed of the inseparable particles, **מ**, **נ**, and the participle **מטעקלם**, or **מטעקלם**, TORTVOSI, INFLEXI, TORTE NAVIGANTES, or HVC ET ILLVC AGITATI. This therefore may be appositely enough rendered, TANQVAM EX IIS QVI HVC ET ILLVC AGITANTVR, INFLEXO CVRSV (vel ITINERE) JACTANTVR, TORTE NAVIGANT, &c. It may also be translated, with sufficient propriety, QVVM HVC ET ILLVC AGITATI fuerint, QVVM TORTE NAVIGAVERINT, &c. WHEN THEY SHALL BE TOSSED (upon

(20) *Saggi di Dissertazion. Accademich. &c. di Cortan. Tom. III. p. 90. In Roma, 1741.*

the sea) HITHER AND THITHER, WHEN WITH MANY TURNINGS AND WINDINGS THEY SHALL PLOUGH THE OCEAN, &c. the particle **ו** sometimes denoting *QUVM*, and a pleonasmus or redundancy of **ו** having not been antiently uncommon, according to (21) Noldius.

The two Maltese stones therefore adorned with this inscription, similar to many others with which we are supplied by the remains of antiquity, must be considered as votive monuments. They were erected by Dionysius and Sarapion, both Tyrians, and the sons of Sarapion, in consequence of a vow, to Hercules, surnamed the CONDUCTOR, or the CHIEF CONDUCTOR, for a prosperous voyage. That the inscription runs in the third person, not the first, as M. l'Abbé Barthelemy asserts, from the correspondent Greek one, and the considerations already offered in support of this point, seems sufficiently clear. This the last word **יְבָרֵכֶם**, BENEDICET ILLIS, not NOBIS, HE WILL BLESS THEM, not US, or BENEDICAT IPSIS, MAY HE BLESS THEM, not US, must be allowed also indubitably to prove.

XI.

If the preceding observations should meet with the approbation of the Royal Society, the following translations of the inscription now before me may perhaps not prove unacceptable to the learned. [See the Inscription, TAB. XI.]

(21) Christ. Nold. *Concordant. Particular. Ebræo-Chald.* p. 353, 354, 470. *Jenæ*, 1734.

לֹאדָנָן לְמַלְקָרְתָּ בַעַל צֶרֶם אִם נָדָר
עָבְרוּ עַבְדָּאֶסֶר וְאֶחָיו אֶסְרִימֶמֶר
מִן בֶּן אֶסְרִימֶמֶר בֶּן עַבְדָּאֶסֶר כִּמְמַע
קֹלָם יְבָרְכֵם

DOMINO NOSTRO MELCARTHO DEO (tutelari) TYRI
MATRIS VOTVM
FECERVNT ABDASARVS ET FRATER (ABDASARI)
ASERIM-HAMMARVS
IS QVI FILIVS (est) ASERIM-HAMMARI FILII AB-
DASARI—IIS
TORTE NAVIGANTIBVS (vel HVC ET ILLVC INFLEXO
CVRSV AGITATIS) BENEDICAT.

ABDASAR AND HIS BROTHER ASERIM-HAMMAR
WHO (also) IS THE SON OF ASERIM-HAMMAR THE
SON OF ABDASAR HAVE MADE A VOW TO MEL-
CARTHUS (OR HERCULES) THE (tutelary) GOD OF
TYRE THE METROPOLIS—IN THEIR TURNINGS
AND WINDINGS (OR IN THEIR CROOKED NAVIGA-
TION) MAY HE BLESS (OR PROSPER) THEM.

That M. l'Abbé Barthelemy's explication of the
inscription here considered is at least somewhat in-
volved, from the foregoing remarks, seems suffi-
ciently clear; whether or no his obscurity, or mistakes,
if any such there be, are removed by the illustration
now offered, the Royal Society will be the best able
to decide.

But though M. l'Abbé has perhaps not arrived at
a complete interpretation of this inscription, he has
nevertheless thrown much more light upon it than
either

either M. le Commandeur (22) de Marne, or M. l'Abbé Fourmont, or indeed any other person who attempted, before his memoir was read, an explication of it. This cannot but be acceptable to the lovers of antiquity, and must intitle him to the thanks of the learned world.

XII.

The language of the inscription is a mixture of Hebrew and Syriac. The first word, **לארנן**, must be looked upon as Syriac; as may likewise the sixth term, **נרר**, on account of the sense wherein it is used. The seventh is Hebrew, as well as Syriac. The ninth, however we render it, is undoubtedly Hebrew; and the eleventh, however this may have escaped M. l'Abbé, as certainly Syriac. The two last words are manifestly Hebrew, though in the last syllable of the former of them *Yod* is suppressed. But this is intirely consonant to the Phœnician form, the coins struck at Sidon generally exhibiting **לצרנס**, for **לצרניס**, with the *Yod* expunged. The term **בן**, SON, used twice in this inscription, is here also apparently Hebrew. We cannot therefore infer from the monument under consideration, as (23) M. l'Abbé Barthelemy has done, that “there is scarce any difference at all between the Phœnician and Syriac languages.” Nor will M. (24) de Guignes me-

(22) *Saggi di Dissertazion. Accademich. &c. di Corton.* Tom. I. Par. I. p. 25—34. In Roma, 1735. & Tom. III. p. 89—111. In Roma, 1741.

(23) M. de Guign. ubi sup. p. 47.

(24) Id. ibid. p. 60.

rit the attention of the learned, when he is pleased to assert, that “ M. l’Abbé Barthelemy has actually “ proved, from this inscription, that the Phœnician “ language is nothing else but the Syriac tongue.”

XIII.

As the most antient Phœnician language was almost intirely the same with the (25) Hebrew, the Syriac words that occur in this inscription, together with what has been already remarked of the forms of the letters it contains, announce it to have been of a later date. The figure of the *Koph* in particular agrees in all respects with the form of the same element exhibited by a coin struck at Achola, or Achulla, as the name appears on this medal, in the Augustan age. That at this time, and even earlier, as well as later, several Syriac words should have been used by the Phœnicians of Tyre, can be no matter of surprize, when we consider, that the Jews themselves, during this period, spoke a language extremely similar to, if not almost intirely the same with, the Syriac.

XIV.

I must beg leave farther to remark, that, by the assistance of the monument now before me, two Phœnician proper names have been discovered, which have never hitherto in any of the antient historians occurred. As the *Aleph* in 𐤀𐤍 was, however, sometimes pronounced like E, and perhaps I, the word

ABDISSAR, (26) met with by M. l'Abbé Barthelemy on an antient coin, and ABDASAR, exhibited by the Maltese stones, may by some possibly be considered as nearly the same name. Should this prove really the case, M. l'Abbé must be allowed to have been extremely lucky in meeting with a proper name so similar to, or rather scarcely distinguishable from, one preserved in an inscription, he was just going to explain. Be this as it will, the word ABDASAR appears, as a part of another Phœnician inscription, on a piece of marble, found amongst the ruins of Citium; which was presented by Charles Gray, Esquire, member of Parliament for Colchester, and fellow of the Royal Society, a gentleman of great merit and erudition, to the University of Oxford.

XV.

To what has been here advanced it may not be improper to subjoin an alphabetic table of the Phœnician letters forming the Maltese inscription, which M. l'Abbé Barthelemy has lately attempted to explain; [Vide TAB. XI.] and on which, in this paper, I have been endeavouring to throw some additional light.

The form of the *Thau* in the table, not bearing the least resemblance to a cross, approaches pretty near that of *Tzade* (27), as exhibited by several of my

(26) *Mem. de Litter. &c.* Tom. XXVIII. p. 597. A Paris, 1761.

(27) Of all the letters in the Phœnician alphabet none perhaps has a greater variety of forms than *Tzade*. One of these, that not seldom occurs upon the Tyrian and Sidonian coins, pretty much resembles the character which M. l'Abbé Barthelemy takes

Tyrian and Sidonian coins; though these characters, as upon inspection will appear, are sufficiently distinguishable from each other.

for *Thau*, in the Phœnician inscription here explained. Now this very figure of *Tzade* immediately precedes the numeral characters in the exergues of several Sidonian coins, and is itself immediately preceded by the letter *Schin*. Those two elements therefore, as occurring on the medals of Sidon, and preceding a date, I took for the initial letters of the words שנת צרן, THE YEAR OF SIDON; and evinced this by such reasons as, I apprehended, could not be easily overthrown. But M. l'Abbé Barthelemy believes the two elements to form the word שר, YEAR; and has been followed in this notion by M. Pellerin, who seems a little to exult, and triumph, on the occasion. However, I still am fully convinced of the truth of what I formerly advanced; and am hindered from coming into M. l'Abbé's opinion, by the following considerations.

1. The very character I took for *Tzade* is the first letter of the words צר, TZOR, or TZVR, and צידן, TZIDON, TYRE and SIDON, on several Tyrian and Sidonian coins. This directly evinces the point in question. Some of these coins are now in my possession, very well preserved, and undoubtedly genuine.

2. The word שר, ANNVS, YEAR, or THE YEAR, does not occur in any of the oriental languages, or dialects, that I have hitherto been conversant with; the term שתא, which is rarely used, being a different word. Nor can M. l'Abbé authenticate the pretended שר, by observing, or rather without any foundation roundly asserting, that as from בנת is deduced בר, so from שנת may be derived שר, SCHAT. For we certainly know, that there is such a word as בר, whereas שר has not hitherto been found. We are not at liberty to frame terms out of our own heads, in order to serve an indefensible hypothesis; nor will a grammatical conjecture, as I apprehend, realize a non-entity.

3. The numbers expressed by Phœnician numeral characters on certain coins that indisputably belong to Sidon do not amount to 112. From whence, as I formerly observed, we may infer, that the æra referred to by those coins was the later epoch of Sidon. Which if we admit, any year deduced from that æra

The

The figure of the *Kph* here is by no means the most antient representation of that element. It

may, with the utmost propriety, be styled THE YEAR OF SIDON ; and that appellation may be more naturally supposed to be pointed at by the letters *Schin* and *Tbau*, prefixed to the dates on the small Sidonian coins, than the single word at length denoting THE YEAR.

4. On one or two medals in my possession, the element *Schin*, as the initial letter of 𐤌𐤍, THE YEAR, OR IN THE YEAR, not the whole word denoting THE YEAR, appears before the numeral characters in the exergue. From whence it seems clearly to follow, that the elements preceding those characters ought to be taken for initial letters, and consequently that the character I denominate *Tzade* ought to be considered in that view.

5. The year exhibited by the first medal in my ^a plate of Phœnician coins, allowed by M. l'Abbé Barthelemy himself to belong to Sidon, and adorned with a numeral inscription, denoting THIRTY-SIX, cannot be the thirty-sixth year of the æra of Seleucus, because the Sidonians were then subject to Ptolemy Philadelphus, king of Egypt, in whose territories the supputation according to that epoch did not take place. The æra then to which the date on this coin refers must undoubtedly be the proper æra of Sidon, which commenced in the year of Rome 643. Nothing therefore can be more natural than to suppose, that the two alphabetic characters preceding the numeral inscription should be the initial letters of the words 𐤌𐤍 𐤔𐤌, THE YEAR OF SIDON, as I formerly ventured to suggest. From whence it seems also clearly to follow, that the notation by me considered in a former paper did not prevail at Sidon before the 207th year of the æra of Seleucus, nearly coincident with the 643d of Rome.

6. If what has been here remarked should meet with the approbation of the learned, they will perhaps not so readily admit M. l'Abbé Barthelemy's pretended coins of Marathus to have appertained to that inconsiderable city. Besides, should this be allowed, which, I am persuaded, it will not, the notation they exhibit cannot well be supposed to have been introduced there before the later Sidonian æra commenced. For if the numeral

^a See *Philophr. Transact.* Vol. L. P. ii. Tab. XXXI. p. 791.

too much resembles the square or Chaldee *Koph*. The character denominated *Koppa*, visible on the

characters above-mentioned were not received in so large, opulent, and polite a city as Sidon, before the year of Rome 643; it is utterly improbable, that they should have been used at Marathus, or any other obscure place of Phœnicia, before the commencement of that year. Nay, it is highly probable, that the introduction of them there was posterior to it. Now if these numeral characters were first received at Marathus in the year of Rome 643, or rather a little after that year, some of M. l'Abbé's pretended coins of Marathus were struck there in the days of Strabo. But then, according to that excellent^b author, the city was destroyed, and it's territory occupied by the Aradians, amongst whom it was divided by lot; so that the foregoing supposition is, at first sight, manifestly absurd. Farther, the word on these medals taken by M. l'Abbé to denote Marathus is frequently not מרת, as he supposes, but מרתב, though on some few of them part of the last letter only appears. This M. l'Abbé, without any manner of foundation, seems to think a new form of the *Ajin*^c, and believes it to be the initial letter of the name of a month; though he had before, in a great measure at least, exploded the notion of such initial letters. In fine the considerations now submitted to the judgment of the learned absolutely determined me to cancel part of a sheet of a small work, put to the press here, in 1753; wherein I asserted, and endeavoured to prove, that those coins belonged to Marathus. This will be attested by the workmen I employed, and the IMPRIMATUR given me by the Rev. Dr. Brown, Master of University College, our worthy Vice-Chancellor, at that time. The cancelled part of a sheet is still in my hands.

7. In farther eviſion of what has been advanced, relative to the initial letters in the exergues of certain Sidonian coins; it may not be improper to observe, that a medal in my small collection exhibits the letter *Hbeth*, immediately after the numerical inscription in the exergue. This probably represents the word חצי, DIMIDIUM, HALF; as both that term and רבע, or רבבה, QUADRANS, QUARTER, are expressed at length on some

^b Strab. *Geograph.* Lib. xvi. p. 1093. Amstelædami, 1707.

^c *Journ. des Sav.* Aout 1760. p. 275. A Amsterdam, 1760.

^d See Plate Fig.

medals of Croton, Corinth, and Syracuse, (28) as well as upon an inedited Punic coin in my small cabinet, was used for *Koph* by both the Phœnicians and Carthaginians in the earlier times. The form of the *Koph* likewise on a Punic medal, (29) that I formerly attempted to explain, was of a pretty high antiquity amongst those nations. Nor does (30) M. Pellerin merit any great attention, or regard, when he assigns that letter the power of *Aleph*; the character on the coin he refers to on this occasion seeming not to point out *Aleph*, but *Koph*. Nor has he so much as offered to interpret the greatest

of the * rarer Samaritan coins. Hence it should seem incontrovertibly clear, that the two Phœnician elements, *Y W*, prefixed to the same sort of numerical characters, on similar medals, must be viewed either in the same or a similar light; which, in conjunction with the initial letters preserved on the famous Samaritan medal of Bologna, formerly mentioned †, must set the point here insisted on beyond dispute.

8. From the preceding observations it seems manifestly to appear, not only that the alphabetic character immediately prefixed to the aforesaid numerals is *Tzade*, but likewise that the coins on which these are impressed must be of a later date. Hence we may conclude, that the times in which those pieces were struck may, with a sufficient degree of precision, be ascertained.

(28) Joan. Bapt. Biancon. *De antiq. Lit. Hebræor. & Græcor. Libel.* p. 57, 63. Bononiæ, 1748.

(29) *De Num. quibusd. Sam. & Phœn. Dissert.* p. 86, 87. Oxoniæ, 1750.

(30) *Recueil de Médailles de Peuples et de Villes, &c.* Tom. III. p. 141, 142. A Paris, 1763.

* *Numism. Antiq. &c.* à Thom. Pembr. & Mont. Gomer. Com. *Collec.* P. 2. T. 85. Num. 7. Adr. Reland. *De Num. Veter. Hebræor. Tab. Non.* Num. 3. p. 202. Trajessit ad Rhenum, 1709.

† *Philosop. Transact.* Vol. L. P. ii. p. 792.

part of the legend, to which it belongs. 'Tis certain, that this character, as well as that on the Punic medal above-mentioned, bears so perfect a resemblance to the figure of *Koph* preserved by a coin of Cosyra, explained by the learned Sig. Abate Venuti, that it cannot well be taken for any other letter. As the objection therefore (31) offered by M. Pellerin to my notion of this character is destitute of every support, it must fall to the ground of course; and consequently no farther defence of that notion can be deemed requisite, or expected from,

S I R,

Your most obedient humble servant,

Ed. Ch. O. n.
April 29, 1763.

John Swinton.

(31) *Raccolta di Medaglie d'Peuples et de Villes*, &c. Tom. III.
p. 141, 142, A Paris, 1763.

XXIII. *A Catalogue of the Fifty Plants from Chelsea Garden, presented to the Royal Society by the worshipful Company of Apothecaries, for the Year 1763, pursuant to the Direction of Sir Hans Sloane, Baronet, Med. Reg. et Soc. Reg. aliquando Præses: By John Wilmer, M. D. clariss. Societatis Pharmaceut. Lond. Soc. Hort. Chelsean. Præfectus et Prælector Botanic.*

- Read May 3, } 2051 **A** Chillæa foliis pinnatis supra-
1764. } decompositis laciniis lineari-
bus distantibus. Flor. Leyd. Prod. 175.
- 2052 Achillæa foliis setaceis dentatis, denticulis subin-
tegis subulatis reflexis. Linn. Sp. pl. 896.
- 2053 Achyranthes caule erecto, foliis ovatis, calyci-
bus spicæ adpressis.
- 2054 Aconitum foliorum laciniis linearibus superne
latis lineis exaratis. Hort. Cliff. 214.
- 2055 Echynomene leguminibus subæqualibus lævi-
bus. Linn. Sp. Pl. 714.
- 2056 Angelica foliolis æqualibus ovatis, inciso-terra-
tis. Hort. Cliff. 97.
- 2057 Anthericum foliis carnosissimis subulatis teretibus,
scapo subramoso. Hort. Upsal. 83.
- 2058 Asperula foliis inferioribus senis, intermediis
quaternis subulatis triquetris, floribus quadri-
fidis. Linn. Sp. Pl. 104.
- Vol. LIV. T 2059 Astragalus

- 2059 *Astragalus* (*Chinenfis*) caulescens procumbens,
capitulis pedunculatis, leguminibus prisma-
ticis rectis triquetris, apice subulatis.
- 2060 *Boerhavia* caule diffuso. Linn. Sp. Pl. 3.
- 2061 *Caucalis umbella* trifida, umbellulis trispermis
involucris triphyllis. Hort. Cliff. 91.
- 2062 *Cerastium* foliis lineari-lanceolatis tomentosis,
pedunculis ramosis, capsulis globosis.
Caryophyllus holosteus tomentosus angusti-fo-
lius. C. B. P. 210.
- 2063 *Chionanthus* pedunculis trifidis trifloris. Linn.
Sp. Pl. 8.
- 2064 *Convolvulus* foliis lanceolatis tomentosis, flori-
bus capitatis, calycibus hirsutis, caule erecti-
usculo. Linn. Sp. Pl. 224.
- 2065 *Cytisus* floribus capitatis, ramis decumbentibus.
Prod. Leyd. 376.
- 2066 *Dracocephalum* floribus verticillatis, bracteis
oblongis ovatis integerrimis, corollis majus-
culis mutantibus. Hort. Upsal. 167.
- 2067 *Echinum* corollis flamine longioribus. Linn.
Sp. Pl. 140.
Echium amplissimum folio Lusitanicum. Tourn.
Inst. 135.
- 2068 *Erigeron* pedunculis alternis unifloris. Hort.
Cliff. 407.
- 2069 *Euphorbia inermis* fruticosa, foliis lanceolato-
linearibus, floribus erectis, capsulis glabris.
- 2070 *Euphorbia umbella* quinquefida trifida dictyo-
toma, involucellis ovatis, foliis lanceolatis,
capsulis lanatis. Lin. Sp. Pl. 460.
- 2071 *Ferula* foliis pinnatifidis, pinnis linearibus pla-
nis trifidis. Hort. Cliff. 95.

- 2072 *Ferula* durior seu rigidis et brevissimis foliis.
Bocc. Mus. p. 2. Tab. 76.
- 2073 *Galium* foliis octonis linearibus fulcatis, ramis
floriferis brevibus. Hort. Cliff. 107.
- 2074 *Gypsophylla* foliis lanceolatis subtrinerviis rectis.
Linn. Sp. Pl. 582.
- 2075 *Hypericum* floribus tetragynis, caule erecto
herbaceo simplici, foliis integerrimis semiam-
plexicaulibus.
- 2076 *Hypochæris* calycibus æqualibus hispidis. Hort.
Cliff. 385.
- 2077 *Isatis* foliis omnibus dentatis. Linn. Sp. Pl. 671.
- 2078 *Lantana* foliis oppositis subsessilibus, floribus
racemosis. Linn. Syst. 1116.
- 2079 *Mesembry-anthemum* foliis triquetris acutis.
punctatis distinctis, calycinis foliolis ovato-
cordatis. Hort. Cliff. 220.
- 2080 *Nigella* pistillis quinis, petalis integris, capsulis.
turbinatis. Linn. Sp. Pl. 753.
- 2081 *Nigella* pistillis denis corollam æquantibus.
Hort. Upsal. 154.
- 2082 *Nigella* pistillis quinis, capsulis muricatis subro-
tundis, foliis subpilosis. Hort. Upsal. 154.
- 2083 *Oenanthe* umbellularum pedunculis margina-
libus longioribus ramosis masculis. 62.
- 2084 *Raphanus* (Chinensis) filiquis torosis acumina-
tis foliis integris.
- 2085 *Rudbeckia* foliis lanceolato-ovatis hirsutis, radii
petalis bifidis.
- 2086 *Rumex* floribus dioicis, foliis cordatis amplexi-
caulibus.
- 2087 *Scabiosa* corollulis quinquefidis, foliis dissectis,
receptaculis florum subrotundis. Hort.
Cliff. 31.

- 2088 *Scabiosa* corollulis quinquefidis radiantibus, foliis bipinnatis linearibus. Linn. Sp. Pl. 101.
 2089 *Scabiosa stellata* folio laciniato minor five maritima. C. B. P.
 2090 *Sisymbrium* foliis pinnato-hastatis dentatis, siliquis erectis. Lin. Sp. Pl. 659.
 2091 *Scandix* feminibus nitidis ovato-subulatis, umbellis sessilibus lateralibus. H. Cliff. 110.
 2092 *Scleranthus* calycibus fructus patulis. Flor. Suec. 377.
 2093 *Solanum* caule inermi herbaceo erecto, foliis ovatis integris glabris, umbellis axillaribus.
 2094 *Trifolium* capitulis dimidiatis, foliis quinatis sessilibus, leguminibus polyspermis. Hort. Upsal. 223.
 2095 *Trifolium* leguminibus racemosis nudis monospermis, caule erecto. H. Upsal. 223.
 2096 *Thalictrum* caule folioso fulcato, foliis linearibus carnosissimis. Dalib. Paris. 162.
 2097 *Veratrum* racemo composito, corollis patentissimis. Lin. Sp. 1044.
 2098 *Verbena* tetrandra, spicis capitato-conicis, foliis serratis caule repente. Fl. Zeyl. 399.
 2099 *Veronica* spicis terminalibus, foliis oppositis linearilanceolatis, acute serratis, caule erecto.
 2100 *Viburnum* foliis serrulatis ovatis acuminatis glabris, petiolis marginatis. Linn. Sp. Pl. 384.

XXIV. *Observations on the Eclipse of the Sun, April 1, 1764: In a Letter to the Right Honourable James Earl of Morton, Pres. R. S. from the Rev. Nathanael Blifs, M. A. Savilian Professor of Mathematics at Oxford, and Astronomer Royal.*

My Lord,

Read May 10,
1764.

AS I had reason to believe, from a calculation made from the best lunar tables, that the north-west limit of the annular appearance, in the late great eclipse of the Sun, would pass but a few miles to the South-west of the Royal Observatory at Greenwich, I thought myself indispensably obliged, to leave Oxford, where my employment then called me; and to attend to an observation which might possibly be of some consequence. And I had at the same time an opportunity of paying my duty to their Royal Highnesses Prince WILLIAM HENRY, and Prince HENRY FREDERICK, who had signified their intention a few days before, of honouring the Royal Observatory with their presence on that occasion.

On the 1st of April, soon after eight in the morning, their Royal Highnesses arrived, and were pleased to signify to me, that it was their desire, notwithstanding their presence, that the observations might be made with all possible accuracy. Their Royal Highnesses were also pleased to permit his excellency the

the Neapolitan envoy extraordinary, the right honourable Lord Leigh, Dr. Morton, S. R. S. and several other gentlemen to be present.

Early in the morning the sky seemed to promise to be favourable to us; but before the time when the eclipse was expected to begin, it became so hazy that we almost despaired of making any observation at all. However Mr. Reeve, the assistant observer, was prepared to observe on the triangular leads, with a two foot reflecting telescope made by Mr. Short, and on March 31st, 21^h 5' 3'' apparent time, he saw the first impression made on the Sun's limb by the Moon; the sky being got tolerably clear a few minutes before. Mr. John Bird, mathematical instrument maker in the Strand, with a two feet reflecting telescope made by himself, on the leads over the new chamber, did not see the beginning, by reason of a tremor, until six seconds later. I myself was endeavouring to observe it with an excellent refractor of 15 feet focal length in the great room: but, having at that time a watery defusion on my eyes occasioned by a cold, I was unfortunately obliged to wipe my eye perhaps at the very time of the contact: for at 21^h 5' 3'' when I again applied my eye to the telescope and placed it on the object, the eclipse was sensibly advanced. So that I apprehend the beginning as observed by Mr. Reeve to be very near the truth.

It had been before agreed that Mr. Reeve, to whose eye the reflecting telescope had been adjusted when armed with Dollond's micrometer, should observe the quantity of the lucid parts, as they decreased before the middle, and also as they increased after the

the middle was past; while Mr. Bird and myself, with the old micrometer applied to the 15 foot tube, should measure the Moon's diameter as seen upon the Sun: But unfortunately, some time before the middle of the eclipse, the haziness became so very thick that we lost sight of the Sun for many minutes. But as soon as the clouds began to disperse, Mr. Reeve observed the lucid parts as under, but did not ascertain the time at either observation.

2' 55'', 5—3' 0'', 2—3' 28'', 7—3' 47'', 6 with several others that increased much faster. By a mean of six observations made (as near the middle as the clouds would permit) both by Mr. Bird and myself, the extremes of which did not differ so much as 3'', the Moon's equatorial diameter was found to be 29' 45 $\frac{1}{2}$ '' as seen on the Sun.

As the observations of the lucid parts were made as fast as the numbers of the micrometer could be read off, and as the difference increased but slowly at first, we will suppose the two first observations to have been made not long after the time of the middle, and at the time of the first observation, the Sun was at least eclipsed 10, 9 digits.

The Sun's horizontal diameter, as observed by Mr. Reeve, with the same micrometer, on the day before, and on the morning of the eclipse, was 31' 56 $\frac{1}{2}$ '', being a mean of six observations not sensibly differing.

About 11 o'clock the haziness became so thick that no further observations could be made, nor, at the time when the end was expected, could the Sun be seen.

At the observatory of the right honourable the Earl of Macclesfield at Shirburn Castle, the beginning of the eclipse was observed at $21^h 0' 48''$ apparent time by one observer, and but one second later by the other. And the end was observed at $23^h 56' 10''$; but this last observation is marked as very doubtful, the air being extremely hazy.

If your Lordship should think the above observations worthy of the attention of the Royal Society, and will be pleased to communicate them to that learned body, it will very much oblige,

My Lord,

• Your Lordship's and their most obedient
humble servant,

Nathanael Bliss.

XXV. *Observations on the Eclipse of the Sun, April 1, 1764: In a Letter to the Right Honourable James Earl of Morton, Pref. R. S. from the Reverend Thomas Hornsby, M. A. and Savilian Professor of Astronomy at Oxford.*

My Lord,

Read May 10, 1764. **I** Take the liberty to transmit to your Lordship the following observations of the great eclipse of the Sun on the 1st of April last, in hopes that your Lordship may think them not unworthy of the attention of that learned body, over which you preside.

On the morning of the 1st of April, the heavens were so uncommonly serene, that I could not but flatter myself with the hopes of a sky very favourable to observation. But about eight o'clock a haziness began to appear, and several clouds to arise from the South-west, which at small intervals deprived us of a sight of the Sun; these clouds however went off entirely to the North-east, and the Sun's limb appeared very nicely defined, and without the least undulation, through an excellent refracting telescope of 12 feet focus made by Mr. Bird, to which had been applied a system of eye-glasses similar to those used for reflectors, and the usual aperture of which had been

^{Inch.} contracted to 1, 2. I continued to keep my eye very attentively fixed upon that part of the Sun's limb,

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where



where I expected the Moon would make the first impression; and at $8^h 59' 33''$ apparent time, saw a very slight alteration on the Sun's limb, which I flatter myself may be considered as the time of the beginning of the eclipse very accurately ascertained; since a gentleman then present, with a reflecting telescope of 18 inches, saw that the eclipse was begun about $6''$ later by the same clock.

As the eclipse advanced, I proposed to measure both chords and the quantity of the lucid parts with a reflecting telescope of 9 inches, armed with a micrometer executed by Mr. Dollond himself, and which was very obligingly lent me for that purpose by my worthy colleague the reverend Mr. Bliss. And accordingly at $9^h 37' 17''$ apparent time (as are all the times hereafter mentioned) I measured the chord of the part eclipsed and found it $= 25' 45'', 8$. At $9^h 50' 24''$ the unobscured part of the Sun was $= 13' 17'', 7$; and at $10^h 3' 39'' = 8' 33'', 8$. At the time of which observation the colour of the sky was remarkably changed.

At $10^h 19' 15''$ I determined, by the old micrometer applied to the 12 foot tube, the quantity of the lucid parts to be $3' 45''$: but, as I found such measurements could not be taken with that instrument, without great difficulty, I immediately endeavoured to determine, by the other micrometer, the quantity of the Moon's horizontal diameter as seen upon the Sun, and found it, by a mean of three observations, hardly differing from each other, to be $29' 45'', 1$.

It was formerly a dispute among the astronomers, whether the Moon's diameter did not appear less when viewed upon the Sun, than when seen upon a
darker

darker ground. The observations of Mr. le Monnier in Scotland, in the year 1748, seemed to leave little room for doubt: and it is to be hoped that observations made with larger and better instruments than mine, and in places where the eclipse was annular, or nearly so, will fully settle this point. At the time of the middle here, the Moon's centre was about 39 degrees high, and therefore the Moon appeared under a greater angle to the eye of the observer than if seen from the earth's center, by about 18 seconds. The true horizontal diameter from the above observations was therefore $29' 27''$; which is but $7''$ less than according to the latest and best tables; which tables may perhaps give the Moon's diameter too large, because constructed from observations made with refracting telescopes, through which the diameters, both of the Sun and Moon, must necessarily appear under an angle somewhat enlarged.

About the time of the middle of the eclipse, I carefully attended to the distance of the cusps: because, from some calculations which had been laid before the public, the Northern limit of the annulus was to pass within 4 or 5 miles of this place; and by other accounts I might have expected to have found myself considerably within the path of the annular penumbra. But as nearly as I could estimate by my eye, the distance of the cusps was not less than $\frac{2}{7}$ of the whole circumference of the Sun. The quantity of the Sun's light was now very sensibly diminished; a very distinct halo was seen at the distance of 12 or 14 degrees round the Sun; and we might have expected to have seen the planet Venus, had not the sky been covered with a considerable haziness. As

this haziness continued, I began to fear that I might be prevented from observing the end, and therefore measured, as carefully as unfavourable circumstances would permit, the following chords.

At	11	33	56	chord	=	19	52, 5
	11	40	25	—	=	16	27, 9
	11	42	7	—	=	15	28, 5
	11	45	7	—	=	13	46, 0
	11	46	57	—	=	12	9, 4

But the sky began to clear up; and at 11^h 58' 3" by the clock, or 11^h 54' 20 $\frac{1}{2}$ " apparent time, I observed the end, which I believe may be depended upon to 3 or 4 seconds.

About 20 minutes after the Sun had passed the meridian, I measured the Sun's horizontal diameter with Dollond's micrometer, and found it = 32' 0", 8.

At seven o'clock in the morning, a Fahrenheit's thermometer, made by Mr. Bird and placed in the shade, stood at 42 $\frac{1}{4}$ degrees. At 7^h 32' I exposed another thermometer of the same scale to the direct rays of the Sun. In five minutes the mercury rose from 43 to 59. At 7^h 44' it stood at 67 $\frac{1}{2}$; and about twenty minutes before the eclipse began this thermometer had risen even to 84.

During the time of the eclipse the following observations were made on each thermometer.

Apparent time. h "	Thermometer in the shade.	Thermometer in the Sun.
At 9 11	53	62
9 30	53	
9 38	$53\frac{1}{4}$	$70\frac{1}{2}$
9 52	$52\frac{3}{4}$	69
10 14	$51\frac{1}{4}$	$60\frac{3}{4}$
10 32	$49\frac{1}{2}$	$53\frac{1}{2}$
10 44	49	53
10 50	$49\frac{1}{2}$	$54\frac{1}{2}$
11 20	$51\frac{1}{4}$	59
11 37	$53\frac{1}{4}$	$63\frac{1}{4}$
12 10	$55\frac{1}{2}$	$66\frac{1}{4}$

For several minutes before and after the middle of the eclipse, the air was very sensibly colder.

I am, my Lord,

Your Lordship's most obedient
humble servant,

Oxford, April 30,
1764.

Thomas Hornsby..

XXVII. *A Table of the Places of the Comet of 1764 discovered at the Observatory of the Marine at Paris, the 3d of January, about 8 o'clock in the Evening, in the Constellation of the Dragon, concluded from its Situation observed with regard to the Stars: By Monsieur Charles Meffier, Astronomer at the Depot of the Plans of the Marine of France, at Paris.*

Read May 17, 1764.

	Truetime.	mean time	R. ascension observed.	Northern declinati- on observ.	Longitude ob- served.	Northera latitude observed.
1764	h ' "	h ' "	° ' "	° ' "	° ' "	° ' "
Jan. 3.	9 24 33	9 29 9	236 29 16	58 32 58	2 11 37 16	72 53 48
	15 5 4	15 9 47	239 45 31	58 51 29	14 49 7	74 22 13
	18 48 55	18 53 42	241 56 1	59 2 54	17 9 14	75 19 50
10	16 10 39	16 18 28	305 57 17	44 23 10	29 0 38	60 41 41
	18 33 53	18 41 44	306 22 2	44 6 14	29 15 54	60 18 20
11	6 43 27	6 51 31	308 14 21	42 45 45	2 21 59	58 28 49
	7 30 54	7 38 59	308 20 17	42 42 35	0 26 19	58 23 58
	18 28 33	18 36 48	309 50 42	41 22 54	1 7 31	56 42 7
14	17 7 58	17 17 22	316 59 44	34 37 35	4 23 15	48 8 16
	18 32 53	18 42 18	317 5 44	34 30 37	4 25 22	47 59 50
15	5 43 14	5 52 48	317 53 29	33 39 23	5 8 47	47 6 48
	7 13 15	7 22 51	317 59 44	33 31 50	5 20 11	47 1 47
16	6 35 19	6 45 15	319 28 52	31 51 34	5 22 51	44 44 33
	7 26 11	7 35 58	319 32 37	31 48 41	5 24 58	44 40 44
18	5 40 32	5 51 8	321 52 36	28 45 54	6 9 39	41 4 3
	7 4 45	7 15 22	321 56 21	28 40 52	6 10 45	40 58 8
19	7 42 8	7 53 4	322 57 8	27 17 25	6 29 47	39 19 57
20	6 50 20	7 1 34	323 46 38	26 5 33	6 43 22	37 56 17
22	5 49 50	6 1 38	325 9 52	23 49 1	7 0 56	35 20 54
	6 11 16	6 23 4	325 10 22	23 47 57	7 0 57	35 19 45
29	5 50 59	6 4 23	328 9 59	17 28 48	7 7 46	28 25 8
30	6 2 52	6 16 28	328 23 44	16 42 10	7 1 12	27 36 59
Feb. 4	5 57 53	6 12 8	329 17 41	13 11 58	6 26 10	24 2 24
7	6 30 23	6 44 54	329 33 51	11 6 15	5 50 20	22 0 9
8	6 16 20	6 30 54	329 39 35	10 26 47	5 41 16	21 21 25
11	6 14 49	6 29 28	329 41 27	9 2 46	5 10 5	20 2 34

I have the honour to send you likewise the elements of the theory of this comet, which monsieur Pingré has deduced from my first observations, as follows.

The ascending node Ω	—	—	3 29 20 6
Inclination	—	—	53 54 19
Place of perihelium	—	—	16 11 48
Logarithm of the distance of the perihelium	9.751415.		

Passage by the perihelium 12 February at 10^h 29^m mean time in the meridian of Paris. The motion retrograde.

XXVIII. *A Supplement to Monsr. Pingré's
Memoir on the Parallax of the Sun: In a
Letter from him to the Royal Society,
Translated by M. Maty, M. D. F. R. S.*

Gentlemen,

Read May 17, 1764. **I** Had the honour to send you my memoir on the Parallax of the Sun as deduced from the observations of Venus; give me leave to add to it some remarks which I have since made, and communicated to our academy. The learned Societies of Europe, amongst which yours holds a most distinguished rank, will be the judges of the success of our expeditions. A definitive decision will probably not be formed till after the observation of the transit of 1769; in expectation of further accounts, which may tend towards this decision, there is at least one which I now submit to your examination.

Supplement to my Memoir on the Parallax
of the Sun.

I impatiently expected the first volume of the Philosophical Transactions of the year 1761, where I hoped to find some observations that might determine which of the observations, viz. that of Mess. Mason and Dixon made at the Cape of Good-Hope, or that which was made at the island Rodriguez by Mr. Thuillier and myself, deserved the preference. The first reduces the Sun's Parallax to $8'' \frac{1}{4}$ at most, whereas

whereas the latter increases it to near $10'' \frac{1}{2}$: the difference is too considerable not to deserve an enquiry into its causes. The expected volume is at last come to hand; and my first care has been to examine with the most scrupulous attention the observation made at the Cape. I owe this testimony to truth, that this observation, as well as all the others of Mess. Mason and Dixon, appears to me to have been made with great judgment. An exception might however be made as to the extensive description of an appulse of σ Sagittarii to the Moon's southern limb, which is found page 389. This certainly was more than an appulse, and the star was really immersed at the Cape.

In this same volume, I found two observations, which would be decisive, if time and other circumstances had permitted them to be made with sufficient accuracy. I have very carefully calculated them both. Mr. Maskeline observed at the island of St. Helena, situated at $15^{\circ} 55'$ South latitude, and according to Dr. Halley at $33' 17''$ of time West of the Observatory at Paris; but this determination of the longitude does not seem sufficiently exact. I have compared many observations of Jupiter's Satellite's immersions and emersions made at the island of St. Helena by Mr. Maskeline with the corresponding ones made at Paris at the Marine Observatory by Mr. Messier, and have only found $31' 56''$ for the difference of longitude between the two places; and as the Marine Observatory is $2''$ East of the Royal Observatory, I think I may conclude that the place where Mr. Maskeline observed is only at $31' 54''$ West of the Royal Observatory.

The clouds greatly obstructed Mr. Maskeline's observations; he could make but one that was useful. At $7^h 31' 07''$ in the morning, apparent time, the bodies both of the Sun and of Venus being perfectly well defined, the distance of the nearest limbs was, by means of an object-glass Micrometer adapted to a reflecting telescope according to Mr. Dollond's invention, found to be $1' 44'' \frac{3}{4}$. I have computed that by allowing $10''$ for the horizontal parallax of the Sun, from that phasis to the internal contact of the limbs, there must have passed $34' 52''$ at St. Helena; I have even assured myself, by some other calculations, that, by an alteration of one or two seconds in the parallax, and of several minutes of time in the longitude of St. Helena, the interval which I have just now determined would suffer an increase or diminution but of very few seconds. The internal contact of the limbs must therefore have happened at St. Helena at $8^h 05' 59''$ in the morning. One can't suppose it to have happened later, because $17'$ after, or at about $8^h 23'$, the skies having cleared again, nothing more could be seen, and the external contact was over. This observation being compared with that of Tobolsk, would give $11''$ for the horizontal parallax, which is a little too much. Mr. Maskeline observes, that tho' Venus's limb and the Sun's appeared as defined as could be desired, yet when the artificial internal contact of Venus's limb with the Sun's was made by means of the object-glass micrometer, Venus's limb dilated and contracted itself alternately, getting and losing a small space within the Sun's limb. He adds, that he endeavoured to take it in the middle of this vibration, but dares not affirm that he exactly

actly did so. Let the distance determined by Mr. Maskeline be only diminished by 2'', and his observation will perfectly agree with mine; but in order to make it tally with that of the Cape, it would be necessary to diminish this distance by 10 or 11'', and it is not very likely that Mr. Maskeline should have committed such an error. His observation might likewise be brought to coincide with mine, by making a diminution of 40 or 45'' in the Western longitude of St. Helena, as I just now settled it; whereas it would be necessary to take several minutes from that longitude in order to make the observation agree with that of the Cape, which does not seem possible. Hence, though I don't take Mr. Maskeline's observation to be in itself absolutely decisive, yet I am persuaded that it adds great weight to the exactness of mine.

Fort St. George at Madras is, according to Mr. Hirst, $13^{\circ} 8'$ North latitude and $3^{\circ} 4''$ of time Eastward of Pondicherry, and consequently at least $5^{\text{h}} 12' 54''$ Eastward of our Observatory's meridian. Mr. Hirst's observation is related in the Philosophical Transactions; the interval observed by him between the two internal contacts was $5^{\text{h}} 51' 43''$, greater by $2' 49'' \frac{1}{4}$ than that which was observed at Tobolsk. This would give $9'' 56$ for the Sun's horizontal parallax, a quantity which is about a medium between the Cape observation and mine. The comparison between the time of observing the contacts, and that of the corresponding observations made in other places, gives conclusions so vastly different, that I dare not relate them here. Mr. Hirst gives a sufficient account of his instruments, but he does not say whether his

pendulum or clock was one with weights. He says he regulated his clock by equal altitudes, and then by meridional passages of Spica Virginis, and of the Sun; but why did he leave off the method of the equal altitudes which he made use of at first, and in what manner did he observe these altitudes to determine the passage either of a fixed star or of the Sun over the meridian? It does not appear that he had a quadrant or transit instrument. I am sorry that this uncertainty about the means employed by Mr. Hirst to determine the time of the phases puts it out of my power to make use of an observation, which might otherwise have been extremely useful, had the astronomer been equally well provided with instruments as he appears to have knowledge and zeal. It is to be observed that by increasing or diminishing by $10''$ the duration observed at Madras, the question of the parallax will be decided conformably either to the observation of Rodriguez or that of the Cape.

In the same volume of the Transactions, are some observations of the same transit made at Abo and at Hernosand; the total duration was observed in both places; it may have been lengthened somewhat beyond its limits; but these observations agree at least in this point with all the others that were made in the North, viz. that being compared with the Tobolsk observation, with regard to the duration of the transit, they give above $10''$ for the horizontal parallax of the Sun.

I have likewise lately had the communication of Mr. Rumowski's observation made at Seleginsk in Siberia. I shall not expatiate upon the particulars here,

supposing that you Gentlemen have received it. The latitude of Selenginsk is $51^{\circ}6'6''$. I have settled the longitude to be $6^{\text{h}}57'50''$ from the Paris meridian. Some immersions of the first and second satellite of Jupiter have given me $6^{\text{h}}57'15''$, $6^{\text{h}}57'20''$, and $6^{\text{h}}58'31''$, by comparing the observations of Mr. Rumowski with the tables corrected upon the observations made at Paris and at the Cape, and estimating, as well as I was able, the different effects of the reflectors and telescopes. The 15th of July, Mr. Le Monnier observed the meridional transit of ϕ Sagittarii at $18^{\text{h}}25'38'' \frac{2}{3}$ of his clock, being at $76^{\circ}00'45''$ from the zenith. The preceding limb of the Moon passed at $18^{\text{h}}38'28'' \frac{1}{2}$, or at $11^{\text{h}}01'40''$ apparent time; when the center passed, the distance of the upper limb was at $75^{\text{d}}52'00''$ from the zenith, and the lower at $76^{\text{d}}22'25''$; the threads, which are $5''$ thick, being entirely upon the Moon. σ Sagittarii had passed at $18^{\text{h}}35'22'' \frac{1}{2}$, the distance from the zenith being $75^{\text{d}}22'20''$. I have observed at Rodriguez the immersion of σ Sagittarii at $14^{\text{h}}01'28' \frac{1}{2}$ apparent time, and Mr. Rumowski observed at Selenginsk the immersion of ϕ at $11^{\text{h}}24'51''$ apparent time. Upon comparing all these things together, I find the longitude of Selenginsk to be $6^{\text{h}}57'21'' \frac{1}{2}$ East from Paris. But this goes upon the supposition that the error of the tables has been quite constant during near 7 hours, which cannot be warranted. Lastly, the observation of the eclipse of the Sun made at Selenginsk June 3, 1761, compared with the same observation made at Tobolsk and at Cajanebourg, determines the longitude of Selenginsk $5^{\text{h}}16'41'' \frac{1}{2}$ East from Cajanebourg, and at $2^{\text{h}}34'30''$ East from Tobolsk, consequently $6^{\text{h}}58'22''$ East

pect it. My clock went too slow that day at noon by $2'2''$, so that I ought to have added $1'2''$ to the time of the clock, whereas I subtracted as much from it. This is the reason of the difference in the times. The small variation in the distances of the limbs was owing to a stricter verification of the parts of my micrometer.

I am, with the most respectful esteem,

Gentlemen,

Paris, Feb. 14, 1764.

your most obedient,

humble servant,

Pingré.

XXIX. *An Account of the Transit of Venus: In a Letter to Charles Morton, M. D. Secret. R. S. from Christian Mayer, S. J. Translated from the Latin by James Parsons, M. D.*

Read Feb. 4, 1752. I

Return you many thanks for the great trouble you have taken in procuring Mr. Dollond's telescope for me; which, happening to arrive very opportunely the day before the observation, gave great pleasure to our Serene Elector: a very happy invention which England alone was capable of producing! but at it's coming to my hands I had no small concern, for fear all our apparatus should be rendered vain, as it was constant rainy weather.

A square mount of solid stone which had been made into an arch, in the Electoral garden at Schwefinga by his Highness's order, afforded us a basis; in the middle of which another mount of like form was raised five feet high, which supported the astronomical quadrant: both were covered with a moveable covering, the building being carried round them.

Two other small buildings of the same construction stood near this; in one of which Mr. Dollond's telescope was placed, and in the other the clock; having so easy a communication with one another, that a glance of the eye commanded them all.

The astronomical quadrant, which was $2\frac{1}{2}$ feet radius Paris measure, was made in the year 1758, at Paris by M. Carinivet mechanical operator to the

Royal Academy, and has an English micrometer; having a moveable wheel, and divided into minutes; and by means of a screw to the index of the quadrant, together with the division of the nonnius plate fastened to it, was so applied, that during the whole time of the observation, while the wire of the plummet constantly glided upon the same point of the limb, it might be moved in the limb by a vertical motion in either direction by the alidad alone. This most excellent invention of your's I first brought into Germany, to the best of my knowledge, after I had seen it's power at Paris in the hands of the ingenious M. Le Monier.

Besides the quadrant, Dollond's telescope, and several other astronomical tubes of 6, 8, 13, and 22 feet; we had a Newtonian telescope of 4 feet Austrian measure, with an eye glass of $\frac{1}{7}$ of an inch.

We had a clock made by M. le Paute, a Parisian, very well defended from the rays of the Sun and from the wind, which I accommodated to this business for a month before, in many celestial observations with as much accuracy as I could, and with more success than we could well expect.

The interior contact of the western limb of Venus, with the western limb of the Sun, observed with Dollond's telescope — —	} True time. h 20 53 8

The moment of the egress, wherein the same limb of the Sun after the interior contact first appeared corniculated, most accurately observed with the same telescope, was — —	} 20 53 35

Whence I conclude, that the interior contact happened — —	} 20 53 33 $\frac{1}{2}$

As to the instant of the exterior contact, I send only two observations made by me with certainty, because of the intervening clouds : the first shews the time wherein I distinctly saw through the clouds the certain emerfion of Venus, $\frac{1}{12}$ part of the diameter of Venus, as nearly as I could judge, excavating the limb of the Sun ; the other wherein, from the unlucky clouds, I could no more observe the least vestige either of the emerfion, or exterior contact, or of Venus.

		True time.		
		^h	'	"
The first outer contact	— —	21	9	4
The other time of the certain emerfion	—	24	17	27

The time which I use, and to which I am still attentive, was obtained by a great number of corresponding altitudes of the Sun, both before and after the day of the transit. But in order to the rendering my calculation perfect one thing is to be desired, that the longitude and latitude of the observatory at Schwezinga might be precisely determined, if possible, in the space of a few months.

The Serene Elector, although he was not well the day before, yet from his great love for astronomy was not only present himself, which other kings and princes usually are ; but being of an excellent and ready genius, he instituted several observations with the astronomical quadrant two hours before the egress, having marked the appulses of the four limbs of the Sun and Venus at each immovable wire, in

order to find out the difference of the right ascension and declinations; the weather being serene from about half an hour after five to half after eight in the morning. Which positions, good Sir, if they may be acceptable, I will immediately send you; and likewise some observations upon a total eclipse of the Moon at which his Serene Highness and his whole court were present, together with two very respectable persons of the court, the illustrious Mess. Holrinhausen and de Stanger, both well skilled in astronomical matters, who were appointed to be my assistants in all these observations.

Hence you will easily perceive, that I am attached to the most earnest endeavour of pleasing so great a Prince; who, taking into his protection all kinds of learning, was the first that introduced astronomy into his country, the name thereof being unknown here for so many ages.

It will be a great satisfaction to me if this our observation may obtain a place among the immortal monuments of your Society; that the future class of astronomers may retain a grateful remembrance of the most serene Charles Theodore Elector Palatine: and while I am, learned Sir, in expectation of this favour from the Royal Society, I humbly commend myself to your regard, being,

Your most humble servant,

Dated at Schwezinga,
23 June, 1761.

Christianus Mayer, S. J.

XXX. *Observationes Astronomicae Christiani Mayer, S. J.*

*Illustrissimo ac celeberrimo Viro ac Domino Carolo de Morton, Academicæ Regiæ Londinensis a secretis, S. P. dicit Christianus Mayer, S. J. S. E. P. Astro-
nomus.*

Read May 24,
1764.

Observationem meam eclipsis Lunaris quam hic tibi fisto, quamque circa 20am Martii Parisios jam miseram, ex responso clarissimi viri de la Lande, die 3 Aprilis ad me dato, audio Parisiis erroris 10 minutorum suspectam haberi, quibus ejus durationem justo minorem collegissem: cum enim ex initio et fine certo hujus meæ observationis inveniam quantitatem durationis $2^h 46' 51''$, eam celeberrimus ephemeridum Parisinarum conditor de la Landius putat esse oportere $2^h 56'$. Non potuit, ut scribit laudatus astronomus, eclipsis hæc diei 17 Martii, nec illa 1 Aprilis, ob nubes Lutetiæ observari: potuit fortasse Londini, et Grenovici; quod si ita, omnem facile controversiam diremptam video. Si enim Londini initium hujus eclipsis acciderit $10^h 31'$ circiter, certum est idem in meridiano Parisino 9 minutis orientaliore accidisse $10^h 40' 40''$, ut habent ephemerides Parisinæ; sin minus, si Londini initium hujus eclipsis observatum fuerit $10^h 39' 5''$, peripicuum est errorem 10 minutorum cadere in ephemerides, et nequaquam in observationem meam, quæ hac ratione statutæ meridianorum differentię ubique terrarum optime congruet: siquidem ex aliis primi satellitis
Jovis

Jovis institutis eclipsim comparationibus Schwetzingam invenio 31' in tempore Viennâ occidentaliorem.

Quare cum rumor hic aulam nostram pervaserit, me in observatione eclipseos hujus lunaris 10 minutis aberrasse, ad te, vir celeberrime, mihi confugiendum fuit, quem hisce demissè oro, ut celeri responso, quid actum sit Londini, quid Grenovici, mihi significare digneris.

Rem quoque mihi feceris gratissimam, si ad me perscripseris, quæ sint meliores magisque usitatæ in Angliâ cœlestes ephemerides pro annis futuris, qui tuis me favoribus toto animo demississimè commendo.

Illustrissimæ tuæ dignitatis

* Servus infimus

Heidelbergæ,
17 Aprilis, 1764.

Christianus Mayer, S. J.

Sereniss. Elector. Palatin. Astron.

Immerfiones et emerfiones macularum » in eclipfi
Lunæ diei 17 Martii, Schwefingæ in Palatinatu
prope Heidelbergam, obfervatæ à P. Christiano
Mayer, è S. J. tubo Dollondi 10 pedum.

Tempus verum.

	^h	'	''
Penumbra cœpit	II	2	24
Penumbra denfior		8	11
Initium dubium		13	8
Eclipfis videtur cœpiffe		14	9
Mare humorum stringitur ab umbra		25	22
Mare humorum medium in umbra		27	24
Tycho incipit immergi		29	58
Tycho totus in umbra		30	21
Grimaldus proximus umbræ		31	31
Grimaldus et Schicardus in umbra		34	15
Mare nubium ingreditur umbram		39	30
Gaffendus ad limitem umbræ		41	20
Infula finus medii in umbra		46	35
Landspergius in umbra		47	33
Umbra Keplerum stringit		48	8
Keplerus ultra dimidium in umbra		51	3
Keplerus totus in umbra		52	4
Copernicus stringitur ab umbra		59	15
Copernicus totus in umbra	12	2	26
Mare fœcundatis fere medium in umbra		6	40
Mare tranquillitatis incipit		8	43
Palus fomnii incipit effe in umbra		10	16
Mare tranquillitatis ultra med. in umbra		13	55
Taruntius totus in umbra		18	14
Plinius et Menelaus ad umbram		20	18
Mare ferenitatis stringitur ab umbra		22	23
Proclus			

Tempus verum.
h

Proclus ad umbram	————	12	27	8
Mare Crisium ultra med. in umbra	————		32	44
Mare Crisium seu Caspium tot. in umbra			38	1
Mare serenitatis med. in umbra	————		41	28
Copernicus incipit egredi	————		58	35
Copernicus dimidius ex umbra	————		59	57
Limbus umbræ stringit Copernicum		13	1	57
Menelaus emergit	————		8	21
Manilius videtur egredi	————		11	35
Mare humorum med. ex umbra	————		13	45
Manilius videtur egressus	————		14	51
Mare serenitatis extra umbram	————		17	2
Sinus æstuum et mare humorum tot. ex umbra			20	25
Mare nubium medium ex umbra	————		23	41
Mare Caspium egreditur	————		25	15
Umbra paludem somnii deserit	————		30	38
Tycho emergit cum dimidio mari Crisio			32	30
Tycho certo emergit	————		35	49
Taruntius videtur egressus	————		41	27
Mare tranquillitatis ultra med. emergit			46	36
Langrenus emergit cum Dionysio	————		47	25
Mare fecunditatis fere tot. emergit	————		52	18
Langrenus certo ex umbra	————		55	22
Videtur umbra mixta luci	————		59	11
Finis eclipsis dubius	————	14	0	11
Finis certus	————		1	0
Penumbra residua	————		3	10
Finis penumbrae	————		4	0

Præter has maculas, à me dimensæ sunt phasæ 20:
et tempore maximæ obscurationis 12^h 37', inventa ob-
scurationis

scuratio maxima $23^{\circ}88\frac{1}{2}$ partium micrometri (quod tubo separato 6 pedum accommodaveram) quarum diameter Lunæ inventa est $3313 = 33'49''$; unde meo quidem calculo obscuratio maxima deducitur 8 digit. 39 minut.

Observatio eclipsis Solaris facta Schwetzingæ in observatorio Electorali anno 1764, die civil. N. S. 1ma Aprilis tubo dioptrico 6 pedum micrometro armato, cælo sereno, barometro 27 pollic. 2 lin. præsentibus illustr. et excell. D. D. O Dunne Ministro Galliæ, et Comite de Riancour Legato Saxoniz.

I M M E R S I O N E S.

Tempus verum.	Partes centes. disci ☉ obscur. mensurantes sagittam ad chordam duo cornua secantem normalis.	Partes centesimæ disci ☉ lucidi.
h / "		
21 40 41	} videtur eclipsis coepisse. } duplex cornu sensibile.	
43 2		
48 4		113
51 53		157
56 9		251
22 4 33		418
6 52		449
9 43		508
11 22		536
14 29		611
19 46		725 $\frac{1}{2}$
23 43		1751
25 13		1709
33 16		1468
38 26		1273
42 40		1162
48 6		987 $\frac{1}{2}$
52 40		861
55 14		762
58 7		658
23 1 33		574
3 54		513
8 3		416
10 56		409

10 dig. 25 $\frac{1}{2}$ min.

E M E R S I O N E S.

Tempus verum.	Partes centes. disci ☉ obscur. mensurantes sagittam ad chordam duo cornua secantem normalis.	Partes centesimæ disci ☉ lucidi.
h 23 13 40		414
14 0		421
18 59		540
21 49		621
24 36		705
27 21		777 $\frac{1}{2}$
31 33		890
33 53		986
37 29		1091 $\frac{1}{2}$
40 45		1209
45 58		1367
50 58		1526
53 43		1606 $\frac{3}{4}$
h 57 48		1729 $\frac{1}{2}$
o 2 29		1860 $\frac{3}{4}$
9 4		2062
14 20	494	
17 38	434	
25 17	311	
27 20	259	
33 42	152	
37 16	110	
43 0	finis certus eclipsis.	

Diameter solis in partibus centesimis micrometri probe verificati $3117 = 32' 4'', 8$.

Nondum licuit phases has omnes acuratius examinare; quarum priores immerfionis, in maxima hominum frequentia, minus exactæ sunt.

XXXI. *Observations on the Eclipse of the Sun at Chatham, April 1, 1764, by Mr. Mungo Murray: Communicated to Joseph Salvatore, Esq; F. R. S. in a Letter from Dr. John Bevis.*

Sir,

Read May 24, 1764. **I** Fancy I can now satisfy your curiosity as to a place in the northern limit of the path of the Moon's shadow, in the eclipse we observed at your house; that is, where the lower limbs of the Sun and Moon coincided, by the following abstract from a letter I received from my friend Mr. Mungo Murray of Chatham, a good mathematician, and author of an excellent work on ship-building.

"I am infinitely obliged to you for your kind present of the telescope glasses. I got them most curiously mounted, and, as you said, they make a 12 foot telescope, which takes in the whole Sun nearly. I set my watch by a very good vertical sun-dial, precisely at 9 o'clock, and at 8 minutes after I perceived the Moon just enter on the Sun. About half an hour after 10, the eclipse was barely *annular*, the light of the Sun below the Moon being but just visible, and less than a hair in the telescope. At 55 minutes past 11 the eclipse ended, and left the Sun quite round."

By this, Sir, I think you may safely conclude, that Chatham was not much more than a mile (perhaps less) south-east of the limit; which therefore passed over Rochester-bridge, or very near it.

Your most obedient humble servant,

John Bevis.

XXXII. *Observations and Experiments on different Extracts of Hemlock : By Michael Morris, M. D. F. R. S.*

Read May 24,
1764.

DR. Wade, an eminent physician at Lisbon, having lately communicated to the London Medical Society, a number of cases, in which the extract of Hemlock prepared at Coimbra in Portugal, had been given with extraordinary success, and having sent me at the same time specimens of the successful extract, and also of the extracts of Hemlock prepared at Lisbon and by Dr. Storck's Apothecary at Vienna, which two last-mentioned extracts he had prescribed for the space of three years, in various disorders, to little or no effect; I thought an experimental inquiry into the component parts of these extracts and that used in London might be attended with some useful or curious consequences; more especially as this medicine was near losing its credit intirely, from its little success here in those disorders in which it had been most strongly recommended by Dr. Storck. I think it not unnecessary to premise farther, that the extract prepared at Coimbra is not so moist as the other extracts, and that it has been given for a considerable time at the dose of a drachm and a half twice a day without producing the least disagreeable symptom.

E X P E R I M E N T I.

24 grains of the extract of Hemlock prepared at Coimbra, digested with an ounce of highly rectified spirit of wine for 36 hours in a warm room, gave a brownish yellow tincture; the clear liquor being poured off, a fresh quantity of spirit was added as before, and exposed to digestion for the same space of time; the second tincture was considerably less coloured; this, added to the former tincture, was filtered, and exposed to the air in a warm room until the spirit was intirely evaporated; the dry residuum weighed five grains; on exposing it to the air it became softer, and even moist at the surface.

On pouring some water on the residuum now moist, it was soon tinged of a brownish yellow, which being poured off, and a fresh quantity added at different times, until an ounce and a half of water had been used, there remained some blackish matter not soluble in water, which when dry weighed one grain, did not attract the moisture of the air, melted and burned with a bright flame when exposed to the fire, was soluble in spirit of wine, and had every characteristic of a resin.

The tinged water, which had been separated from this resin and filtered, was evaporated slowly, until a brown dry matter remained weighing three grains, which in a few hours attracted the moisture of the air, and relented into a dark brown thick liquor, of a saline taste, and the smell peculiar to the extract of Hemlock. One drop of this liquor, diluted with a little water, destroyed the colour of ten times the quantity

quantity of syrop of violets, without giving it the least red tint; reducing it, on adding some drops of oleum tartari per deliquium, it suffered no remarkable change. Spirit of salt did not occasion any alteration in it. But with oil of vitriol there was a strong effervescence, without any sensible fume.

It appears from the above experiments, that the Coimbra extract of Hemlock contains one fifth soluble in spirit of wine, $\frac{3}{5}$ ths of which consist of an oily essential salt, the remainder being a resin.

EXPERIMENT II.

The extract of Hemlock from Vienna was softer than that from Coimbra; on breaking it, there appeared small whitish streaks on each surface. 24 grains of it, treated as in the former experiments with spirit of wine, gave a fine deep green tincture, which on evaporation gave a residuum of a dark green towards the edges of the cup and a dark brown towards the middle; the whole residuum when dry weighed two grains and $\frac{1}{4}$; on leaving it exposed to the air, the brown matter attracted moisture from it and relented into a thick brown liquor; on adding water to it, as in the experiments on the Coimbra extract, the solution was of a light green colour; on evaporation it gave $\frac{3}{4}$ ths of a grain of dark brown residuum, which ran per deliquium into a brown liquor, differing only in colour from that obtained by a similar process from the Coimbra extract. The undissolved resinous matter weighed $\frac{1}{2}$ grain, was of a greenish colour, but in other respects like the resin of the Coimbra extract. It appears from the green
tincture

tincture communicated both to water and rectified spirit by the Vienna extract, that the Hemlock had been gathered too soon, and before the plant was in vigour.

E X P. III.

The spirituous tincture of the Lisbon extract was not so green, nor was the green so durable, as that of the Vienna extract: the phænomena, in consequence of the other experiments, did not differ materially from those of the Vienna extract.

E X P. IV.

The spirituous tincture of the extract of Hemlock prepared at the Apothecary's-Hall, was like in colour to that of Coimbra, but the residuum did not differ considerably from that of Vienna and Lisbon. This extract has been used with some success at the Westminster-Hospital.

E X P. V.

The spirituous tincture of the powdered leaves of Hemlock was like in colour to the last; the residuum differed materially from that of the former extracts only in its resin's being considerably more fluid.

These experiments shew that the extract of Hemlock prepared at Coimbra contains a far greater quantity of an essential oily salt and resin, than the other extracts. As the oils, salts and resins are the most active

tive parts of vegetables, may not the well-attested salutary effects of the Coimbra extract be owing to its greater quantity of these active principles, particularly if we consider the large dose it has been prescribed in? As these active oily salts and resins are soluble in spirit of wine, we have the means of obtaining them from the extract of our own Hemlock in sufficient quantities for use, and without fatiguing the stomach with the nauseous inactive parts of the extract.

But as experience alone can shew whether the virtues of the Hemlock reside in the whole extract, or in the saponaceous parts soluble in spirit of wine; I shall content myself with proposing these few hints, until experiments shall enable me to lay the other consequences of these assays with proper weight before the Society.

XXXIII. *Essay on the Use of the Ganglions of the Nerves: By James Johnstone, M. D. Communicated by the Right Rev. Charles Lord Bishop of Carlisle, and F. R. S.*

Read May 31, 1764. **T**HE Ganglions of the intercostal nerves, first discovered by Fallopius, are oblong and very hard bodies; the uses of which have not been satisfactorily ascertained by any one. Few anatomists have indeed entered deep into the subject, except the learned J. M. Lancisi, who imagined them muscles sui generis, and, like other muscles, capable of contractions; by which he thought the nervous spirits were accelerated and impelled with such additional forces, as are by him supposed necessary to the production of motions in muscles subject to the will: And in order to give an idea of the structure of all other Ganglions, he particularly describes and delineates that of the first cervical Ganglion (*a*).

This theory has the misfortune to be erroneous in its foundation. For Haller and other succeeding anatomists have not been able to discover this muscular apparatus in the first cervical Ganglion (*b*). The coats of Ganglions I have found with the appearance and firmness of ligaments; but incapable of such extension or retraction, as elastic muscular fibres always allow of.

(*a*) See Lancisi's *Dissertation* in Morgagni adversar.

(*b*) Hæc, Element. Physiolog. Human. T. iv. p. 203.

Ganglions besides, instead of being instruments subservient to the will, are almost peculiar to nerves, distributed to parts, the motions of which are totally involuntary: And our author must indeed have been greatly misled by his hypothesis not to observe this striking circumstance.

The theory, which prevailed in his time and country, of the action of the dura mater upon the brain, now exploded, might lead this great man more entirely to believe an analogous muscular power in Ganglions. But the brain needs no muscular force to impress motion upon the animal spirits; and granting Ganglions to be, as is ingeniously conjectured by Lancisi and Winslow, subsidiary brains, or analogous to the brain in their office, neither will they need any such muscular apparatus and force. A power, in fine, absurd no less than chimerical, as it supposes the force of muscles of the greatest exertion and effect, to be derived from those of least bulk and strength (which must be in some proportion to the quantity of muscular fibres); and would be a single instance of a mechanical force producing another infinitely greater than itself.

1. Ganglions are observed to be formed generally upon nervous cords formed by the union of several different nerves. 2. They appear to abound with blood vessels. 3. The bulk of a Ganglion exceeds, for the most part, that of all the nerves and vessels, which it receives, and of which it may seem composed (c). Hence we may not unreasonably conclude,

(c) *Gangliorum moles major est quam sit aggregatum omnium vasorum ingredientium atque egredientium; quo fit, ut ad eorum productionem, necesse sit concurrere, præter communia vasa, peculiare aliud corpus, non tam ex cohærentia et*
that

that in Ganglions the different nervous filaments are very intimately mixed, a new nervous organization, or modification of the medullary substance, may take place, so as to secern new animal spirits, or alter the direction of those already brought thither; a conjecture, which has the sanction of the latest as well as the earlier thoughts of the great Morgagni (*d*).

In order to determine the particular use of Ganglions (the intimate structure of which, equally with that of the brain and medullary substance of the nerves, we are hitherto ignorant of) in the animal system; let us try, if something tending this way may not be suggested, by reflecting on the functions and motions of the parts applied principally by nervous cords from below the Ganglions.

The intercostal or great sympathetic nerves abound most of all others with Ganglions (*e*); and by examining what is peculiar in the motions of parts, to which these nerves are distributed, we shall probably be led to the uses of Ganglions.

The muscular substance of the heart has its principal, or rather all its nerves, from the intercostals;

complicatione præfatorum nervorum, ac sanguiferorum, quam ex novis organicis partibus, quas provida solersque natura, subsistentibus probeq; excoctis liquidis, simul etiam elongatis, varieque dispositis, solidorum fibris, fingat et creat. Lancisi, de Gangliis, loc. cit.

(*d*) See Morgagni, adversar. Anatom. ii. p. 71. And De Sedibus et Causis morborum, Epist. xii. art. 14. p. 95, vol. I.

(*e*) Super omnes nervos, intercostali, Ganglia sunt frequentissima, in cervice quidem tria; in thorace, lumbis, et pelvi tot, quot nervorum ex spinali medulla propaggines intercostalis accipit: tum in cordis vicinia, sub diaphragmate, circa arteriæ cœliacæ et mesentericæ originem: et circa renem passim in plexuosis retibus. Haller, Elem. Phys. T. iv. p. 202.

which

which are always detached from the principal cords below the Ganglions, and chiefly from the inferior cervical Ganglion. The few nervous cords from the par vagum or 8 pair, which in the human subject are sent towards the heart, are almost totally spread upon the pericardium and great vessels (f).

In the abdomen this nerve unites with the par vagum of the right side (g), and together form the great semi-lunar Ganglion; from which, and from other Ganglions formed in inferior parts of the abdomen, filaments are distributed to the intestines, the liver, the spleen, the kidneys; and some of them descend to the Fallopian tubes, uterus, and other parts in the pelvis; some of which are also in part furnished with filaments from the lumbar nerves.

The heart and intestines being wholly supplied by nervous filaments detached below some remarkable Ganglion, we must inquire what is particular in the motions of these parts, or in their structure: But the motions of the heart and intestines are remarkable, and exactly analogous in being involuntary, or not liable to be either stopped, renewed, or in any way controuled by the will.

Tho' it be very certain that these motions are excited in the heart by the gentle stimulus of the blood upon the infernal surface of that organ; and in the intestines by that of the secreted liquors, and of the food taken in; of which stimuli these parts have the quickest and most exquisite perception: yet this being ordinarily not so strong as to make us conscious of its action, much less painfully so, can hardly be

(f) Haller, Elem. Phys. T. i. p. 366.

(g) Winflow, Traité des Nerfs, N°. 141.

supposed to render these motions quite uncontrollable by the will, without some other efficient cause (*b*).

Anatomy discovers no peculiarity in the muscular structure of these parts likely to account for this; and, excepting in their nerves having Ganglions, which seem indeed almost appropriated to them, no anatomical difference has been observed, no mechanism, which these parts have more than those muscles, which are subject to the direction of the will.

May we not then reasonably conclude, that Ganglions are the instruments, by which the motions of the heart and intestines are, from the earliest to the last periods of animal life, rendered uniformly involuntary; and that to answer this purpose is their use, which they subserve by a structure unknown to us, no less than that of the brain, though it seems not improbable the first may be analogous to the last?

This conclusion concerning the use of Ganglions is supported by every truly parallel instance. Thus the motions of the uvea, or muscular circle of the eye, ever contracted or dilated as the eye is more or less irradiated with light, are as much involuntary as those of the heart; and it is known to anatomists, that the muscular fibres of the uvea are supplied by nerves from the lenticular Ganglion, which seems

(*b*) In the best explanation of the vital and involuntary motions, which the public has been favoured with, it is remarked by the most ingenious author, “ I imagine, that the mind’s want
“ of power over the motion of the head is not only owing to
“ its being continually acted upon by a stimulus, but in part to
“ an original constitution; and that, tho’ we should suppose this
“ organ for a little while free from every degree of irritation, yet
“ the mind, by an effort of the will, could not move it.” Dr. Whytt’s Essay on the vital and involuntary motions of Animals, p. 316.

formed solely for the use of that muscle, and for that purpose.

That the determinations of the will are, as it were, intercepted, and prevented from reaching certain parts of the body, by the means of Ganglions, may be farther inferred by considering, that all nerves, which have a ready communication with the soul, either by affecting it with perceptions, or conveying its commands, have no Ganglions: These are never found upon the olfactory, optic, or auditory nerves, any more than upon the nerves and instruments in voluntary motion. For we may well imagine the same mechanism, which prevents the will from extending its controul to some muscles, placed upon a sensory nerve, would have equally hindered the conveyance of any sensible impression to the mind.

The left nerve of the eighth pair, distributed to the stomach, and probably the cause of the distinct and exquisite sensation of that organ, and perhaps also principally concerned in transmitting the sense of hunger to the mind, may therefore be reckoned a sensory nerve. It is certain also, that Haller (1) and most modern anatomists do not allow any Ganglion to this nerve, though Winslow does, and Vieussens delineates one not far from the great Ganglion of the intercostal nerve as proper to the eight pair.

If muscles subject to the will might have been totally supplied with nerves, which have Ganglions; the diaphragm had probably been entirely furnished from the intercostals, as most of the parts in the thorax above it and in the abdomen below it are. But as the motions of this muscular membrane were to be

(1) A. Haller, *Primæ Lin. Physiolog.* N°. 377.

controulable by the will, we find peculiar nerves, namely the phrenic, which have no Ganglions sent to it from a great distance.

In proposing this as the probable use of Ganglions, I am far from thinking it entirely exempt from difficulties; but they are chiefly such, as arise from our imperfect knowledge of the nerves in general; a terra incognita, which remains to immortalize the name of some future discoverer in anatomy. It is well known, for instance, that all the nerves sent from the spinal marrow have Ganglions, where they send off the branch, which communicates with the intercostals (*k*). Though this be true, it is most probable, that these Ganglions respect the intercostals, and only affect its nerves, leaving the other fibres fit and free for the conveyance of the commands of the will, as in fact many of them are distributed to muscles under its power and direction.

So likewise we are not to imagine, wherever the nerves unite, that their medullary substance either decussates, or is so intimately mixed, as is reasonably supposed to be the case in Ganglions by most anatomists from Lancisi down to Haller. We know at least, that this is far from being the case in the optic nerves; for though they unite, and were supposed to cross each other, the contrary appears by observations made in the bodies of persons, who were blind of one eye, from a fault in the optic nerve, the nerve of the affected side only being wasted, while the other was large and plump (*l*). And we may justly infer the

(*k*) See the table of Vieussens.

(*l*) See Monro's Anatomy, p. 356; also N^o 23, of the nerves in general.

plexiform unions of the nerves distributed to the superior extremities not more intimate, nor to serve any such purpose as Ganglions, since these nerves are equally motory and sensory, no other nerves being distributed to the skin, the organ of touch, but from the subdivision of these plexuses.

Kidderminster,
Nov. 24, 1763.

James Johnstone, *M. D.*

XXXIV. *An Account of several fiery Meteors seen in North America: In a Letter to John Pringle, M. D. and F. R. S. from John Winthorp, Esq; Hollifian Professor of Mathematics and Philosophy at Cambridge, in New England.*

S I R,

Read June 7, 1764. **I** Am greatly indebted to you for the honour you have done me in sending me your curious account of the late fiery Meteor in Britain, which I received through the hands of my very worthy friend Dr. Franklin. I have perused the account with great satisfaction, in which, as it appears to me, you have determined the figure, magnitude, height, path, and velocity of the Meteor, with as much exactness as the nature of the thing will admit of. The circumstances you mention, p. 259, 260, will, I am afraid, always prevent the attainment of the precision one could wish for in those particulars, so necessary for laying a sure foundation to build a theory upon. The hypotheses hitherto advanced are liable to great difficulties, and the hints you have given in the conclusion towards another appear intirely new, and free from several objections with which the others are embarrassed; and I should be very glad to see them thrown together into a just system. If any observations should occur to me, that might throw light on this difficult subject, I will do my self the honour to communicate them

to you ; and ask leave, for my own information, just to query, what center these bodies may most probably be supposed to revolve round? Either the Earth or the Sun seem to bid fairest for this.

1. A body revolving round the Earth in a circle, at the height of about 40 or 50 miles, would move but 7 miles in a second ; and in a very eccentric orb, near a parabola, but 10 miles : which falls much short of the velocity of your Meteor, which was of 30 miles in a second *.

2. The Earth's annual velocity round the Sun is near 16 miles in a second ; and a body revolving in a very eccentric orb would have, at the same distance from the Sun, a velocity of 22 miles in a second. Wherefore if the Earth and such a body near it were moving in the same direction, that body would get before the Earth with a relative velocity of 6 miles in a second ; but, if moving the contrary way, it would be left behind with a relative velocity of 38 miles in a second : And this is the greatest possible relative velocity. Bodies moving in oblique directions may have any relative or apparent velocity less than this maximum. This supposition therefore agrees better with a velocity of 30 miles in a second, than the former ; but I shall be much obliged to you for your thoughts on this point.

As to our late Meteor of May 1759, I have not been able to come at any farther particulars than what are contained in my letter to Dr. Birch, excepting only as to the loudness of the report, at a great distance from the place of explosion. An intelligent man of this town has since informed me, that he was then fishing in a boat at anchor about a

* Vid. *Philos. Trans.* Vol. LI. p. 263.

mile below the light-house at the entrance of Boston harbour, and heard an uncommon noise, which was somewhat like that of a very hard clap of thunder at a great distance, tho' there was not a cloud to be seen. All the company in the boat were so startled at it, that they left off fishing to attend to it; and the noise increased to such a degree as amazed them all. He says, it seemed like a continued succession of volleys of small arms. He thinks it lasted about 3 minutes, and gradually went off towards the South-East. They took it for the noise of an earthquake, and expected to find every body talking of one, when they got ashore; tho' they could not perceive the least agitation in their boat.

Had this Meteor happened in the evening, instead of the day-time, it would have had many more observers; and the brightness of it would probably have been thought as extraordinary as the noise.

On this occasion I take the liberty to give an account of two or three other Meteors of this sort, seen in North America; which, if they are permanent revolving bodies, according to your hint, p 273. may possibly be of some service hereafter in enumerating them. The first mentioned in the inclosed paper I saw in this town; the second, I received an account of in a letter from the Rev. Mr. Clap, President of Yale College in New-Haven, who heard the noise himself, though he did not see the light; and the account of the third, here transcribed in the very words of the original, was given me at St. John's, Newfoundland, by Michael Gill Esq; Chief Judge in the courts there, when I went thither to
view

view the transit of Venus last June. None of these accounts are circumstantial enough to ascertain the necessary particulars of magnitude, height, and velocity. All I can collect from them is, that the height must have been very great, and consequently the explosion very great likewise, to produce such a report in a highly rarified medium.

Allow me, Sir, the honour of subscribing myself, with very great respect,

Your most obedient

Cambridge, New-England, Humble servant,
November 17, 1761.

John Winthrop.

*Account of Three Meteors seen in North-
America.*

Read June 7, 1764. **O**N the 3d of June 1739, as I was walking over the common in this town [Cambridge] about 10 o'Clock in the evening, the Moon, which was newly past the first quarter, shined bright, and but few clouds to be seen, I was on a sudden surpris'd with four or five flashes of light, succeeding each other as quick as possible. This I at first took to be lightening; but, looking up, presently discovered the cause of it, which was a large Meteor moving almost in the meridian from South to North. The body of it was very bright, and left behind it
several

several sparks or lesser balls of light. When I first saw it, it was not far from the zenith, from whence it moved, not very swiftly, till at about the height of 30° above the horizon it expired. In about $2'$, it was followed by an hollow rumbling noise, pretty loud, and so much like remote thunder, that several persons in their houses, who did not see the Meteor, took it to be thunder; as others, within doors, who saw only the flash, and not the body of the Meteor, thought it lightened. But as there was no thunder nor lightening before or after, nor any clouds likely to produce them, as I was well assured, being then abroad, I question not but this report was occasioned by the explosion of the Meteor. And this is confirmed by the great extent of this sound, which was heard in several places above 80 miles distant from each other. And from hence, as well as from the length of time between the light and the noise, it may be collected that the Meteor must have been very high in the atmosphere.

II. A Meteor on the 24th of November 1742, in the Southern parts of New England.

In New Haven, in Connecticut, one man saw a ball of fire about 4 or 6 inches in diameter, passing along from the South-west to the North-east, and a stream of white, bright, and clear fire followed it, of near the same bigness, and of considerable length. Then the ball broke into sundry small pieces, and vanished with a kind of flash; and, a full minute after, he heard a noise, much like that of rumbling thunder, and, he says, about as long again as a clap of thunder usually is. — Sundry people at Rehoboth, in this province [Massachusetts] saw a ball of about
a foot

a foot diameter, toward the West from them, and it fell to the ground. — At New-London, in Connecticut, the stream of fire appeared in the North or North-west; and some who were off at sea, near New-London, took the noise to be from great guns at New-London battery. — Mr. Clap observes, that, though the informations he had received differ as to particular circumstances, thus much in general seems to be certain, that people in most, if not all the towns between Norwalk, near the West end of Connecticut, and Braintree near Boston, which is at least 200 miles, heard an unusual noise in the air, like thunder or the discharge of a cannon; and sundry people, in most places, about a minute or more before the noise, saw a ball or stream of fire in the air, moving in some form or other. It was cloudy in the morning, but about 11 of the clock, when this phenomenon happened, it was generally clear, and but few clouds to be seen.

III. A Meteor on the 4th of May 1760, at Newfoundland.

The deposition of James Cawley, master of the Sloop Content, taken before Michael Gill Esq; one of his Majesty's Justices of the peace for the district of St. John's, Newfoundland, sayeth, that, coming from the Banks of Newfoundland for this harbour of St. John's, being Sunday the fourth instant, about a quarter before twelve o'clock at night, being calm and the weather very clear and fair, then, near the mouth of this harbour, a sudden light shined, at which time we saw a fiery Comet or Meteor in the air, at first appearing in the shape of a flask or Florence bottle, which as it came nearer to us still increased
in

in magnitude, making the air very hot, shooting from the North-ward to the South-west; sparks of fire darting from it, the bigness of a man's fist. It came very near us before it disappeared, when it seemed to us to be of the bigness of a ship's boat, with a long tail extending from it, attended with a noise like thunder. As it came near the water, the body appeared as black as pitch and then vanished; the tail remaining some minutes before it disappeared.

Signed,

James Cawley.

Sworn before Michael Gill,
the 15th of May, 1760.

The above was likewise attested upon oath by the subscribers, being failors on board the said vessel, and at that time upon deck.

John Sullivan,

[A true Copy.]

Mark

Thomas + Frog.
of

The testimony of Richard King, which he is ready to make oath to, faith, that coming from Kitty-Vitty to St. John's on Sunday the 4th of May, between the King's Bridge and the Garrison, he saw towards the garrison as if there was a star shooting, falling greatly, but only it made too large *. It was as large as a man's head, and just before it came to the ground it broke all to pieces, which made like large sparks of fire flying from it; and in that time it was as light as ever he saw all day: And in less than two or three minutes there was a rumbling noise in the air, something like thunder.

Several other persons in St. John's were prodigiously surpris'd at the same light.

* His meaning seems to be, it was too large for a star.

XXXV. *Some New Properties in Conic Sections, discovered by Edward Waring, M. A. Lucasian Professor of the Mathematics in the University of Cambridge, and F. R. S. to Charles Morton, M. D. Sec. R. S.*

THEOR. I.

Read June 21, 1764. SIT ellipsis APBQCRDSET, &c.

describantur circa eam duo polygona [TAB. XIII. Fig. I.] ($abcdef$, &c. $pqrstv$, &c.) eundem laterum numerum habentia, & quorum latera ad respectiva contactuum puncta (APBQCRDS, &c.) in duas æquales partes dividuntur, i. e. $aA = Ab$, $bB = Bc$, $cC = Cd$, &c. $pP = Pq$, $qQ = Qr$, $rR = Rs$, &c. & erit summa quadratorum ex singulis unius polygoni lateribus æqualis summæ quadratorum ex singulis alterius polygoni lateribus, i. e.

$$ab^2 + bc^2 + cd^2 + de^2 + ef^2 + \dots = pq^2 + qr^2 + rs^2 + st^2 + tv^2 + \dots$$

Cor. Ducantur lineæ AB, BC, CD, DE, EF, &c. PQ, QR, RS, ST, TV, &c. & erit $AB^2 + BC^2 + CD^2 + DE^2 + EF^2 + \dots = PQ^2 + QR^2 + RS^2 + ST^2 + TV^2 + \dots$

THEOR. II.

Idem positis fit O centrum ellipseos, & ducantur lineæ $OA, OP, OB, OQ, OC, OR, OD, OS,$ &c. erit

$$OA^2 + OB^2 + OC^2 + OD^2 + \&c. = OP^2 + OQ^2 + OR^2 + OS^2 + \&c.$$

Cor. Ducantur etiam lineæ $Oa, Op, Ob, Oq, Oc, Or, Od, Os,$ &c. & erit

$$Oa^2 + Ob^2 + Oc^2 + Od^2 + \&c. = Op^2 + Oq^2 + Or^2 + Os^2 + \&c.$$

Hæc etiam vera sunt de polygonis inter conjugatas hyperbolas eodem modo descriptis.

THEOR. III.

Sit conica sectio $MPQRSTM'$ &c. [Fig. 2.] cujus diameter fit AL , et ejus ordinata ML' ; sit $Mp = Mv$, & consequenter $Lp = Lv$.

Ducantur lineæ $pq, qr, rs, st, tv,$ &c. quæ respective tangant conicam sectionem in punctis $P, Q, R, S, T,$ &c. & erit contentum

$$pP \times qQ \times rR \times sS \times \&c. = Pq \times Qr \times Rs \times St \times Tv \times \&c. \text{ vel, quod idem est, summa omnium hujus generis rationum } (Pp : Pq, Qq : Qr, Rr : Rs, Ss : St, \&c.) \text{ erit nihilo æqualis.}$$

Cor. 1. Sit ellipsis $PQRSTV$ &c. circa eam describatur quodcunque polygonum ($pqrstuvw,$ &c.),

[Fig.

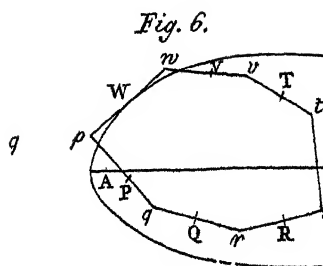
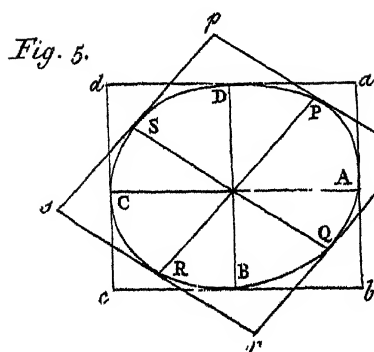
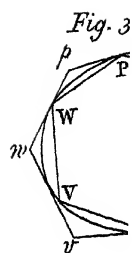
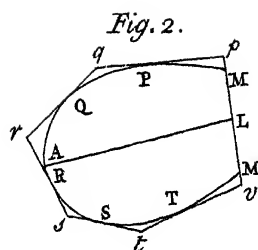
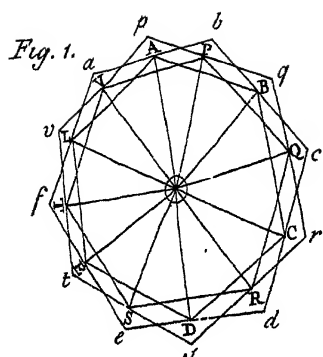


Fig. 4.

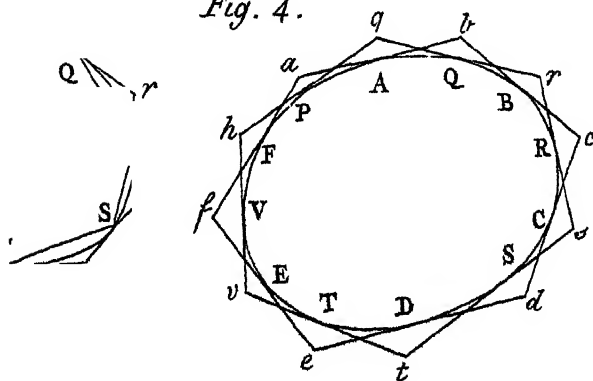
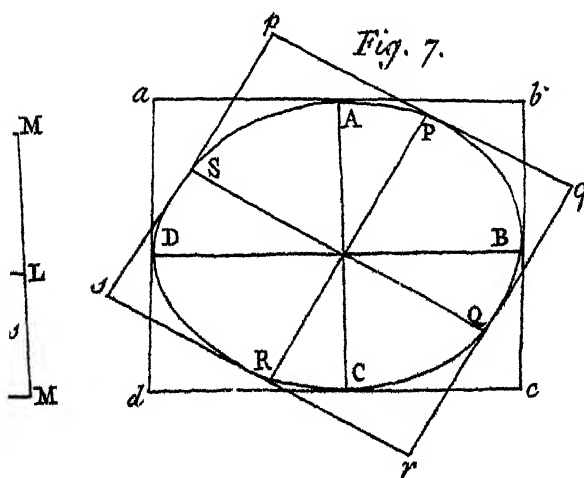


Fig. 7.



[Fig. 3.] cujus latera respective tangant ellipsim in punctis P, Q, R, S, T, V, &c. & erit contentum

$$pP \times qQ \times rR \times sS \times tT \times vV \text{ \&c. } = Pq \times Qr \times Rs \times St \times Tv \times Vw \times \text{ \&c. }$$

Cor. Ducantur lineæ PQ, QR, RS, ST, &c. & pro finibus angulorum WP p , QP q , RQ r , QR r , SR s , TS t , &c. scribantur respective a, p, b, q, c, r, d, s , &c. & erit

$$abcd \text{ \&c. } = p q r s \text{ \&c. }$$

Et sic de polygonis inter conjugatis hyperbolas inscriptis.

Idem verum est de polygono, cujus laterum summa vel area minima fit, circa quamcunque ovalem in sese semper concavam descripto, ut constat e nostra Miscell. Anal.

T H E O R. IV.

Sit ellipsis PAQBRCSDTEVF, &c. [Fig. 4.] circa eam describantur duo polygona $abcdef$, &c. $pqrstuv$, &c. eundem laterum numerum habentia; eorum latera ab, bc, cd, de, ef , &c. pq, qr, rs, st, tv , &c. respective tangant ellipsim in punctis A, B, C, D, E, F, &c. & P, Q, R, S, T, U, &c. & fit $aA : Ab :: pP : Pq$, & $bB : Bc :: qQ : Qr$ & $cC : Cd :: rR : Rs$ & $dD : De :: sS : St$, & sic deinceps. Et area polygoni $abcdef$, &c. æqualis erit aræ polygoni $pqrstuv$, &c.

Cor. Duo parallelogramma ($abcd$ & $pqrs$) circa datæ ellipseos conjugatas diametros (AC & BD; PR, QS) [Fig. 5.] descripta, erunt inter se æqualia.

In hoc casu enim $aA = Ab$, $bB = Bc$, $cC = Cd$,
 $dD = Da$, & $pP = Pq$, $qQ = Qr$, $rR = Rs$,
 $sS = Sp$; & consequenter $aA : Ab :: pP : Pq$ &
 $bB : Bc :: qQ : Qr$, & sic deinceps: ergo per the-
 orema hæc duo parallelogramma erunt inter se æqualia,
 quæ est notissima ellipseos proprietas.

Idem dici potest de polygonis inter conjugatas hy-
 perbolas eodem modo descriptis.

THEOR. V.

Rotetur conica sectio circa diametrum ejus (AL),

& fit MAM' , &c. solidum exinde generatum; sint
 pq , qr , rs , st , tv , vw , wp , &c. [Fig. 6.] lineæ,
 quæ tangant solidum in respectivis punctis P, Q, R, S,
 T, V, W, &c. & erit contentum

$$pP \times qQ \times rR \times sS \times tT \times vV \times wW \times \&c. = \\ Pq \times Qr \times Rs \times St \times Tv \times Vw \times \&c.$$

THEOR. VI.

Sit ellipsis $APBQCR$, &c. rotetur circa diame-
 trum ejus BD ; & circa conjugatas diametros (AC
 & BD , PR & QS) describantur elliptici cylindri
 ($pqrs$ & $acbd$) [Fig. 7.] solidum generatum cir-
 cumscribentes, & erunt hi duo cylindri inter se æ-
 quales.

Sint duo solida e truncatis conis composita, solidum
 generatum circumscribentibus, & quorum latera con-
 tinuo

tinuo eâdem ratione ad puncta contactuum dividuntur; erunt hæc duo solida inter se æqualia.

Et sic de solidis inter conjugatas hyperboloides eodem modo descriptis.

Facile constant plures confimiles conicarum sectionum proprietates.

Hujus generis proprietates affirmari possunt de infinitis aliis curvis, ut facile deduci potest e nostrâ Miscell. Anal.

XXXVI. *An Account of the Effects of
Lightening at South Weald, in Effex:
By W. Heberden, M. D. and F. R. S.*

Read June 28, 1764. **S**OUTH Weald is a village in Effex, about eighteen miles distant from London, and two to the North West of Brentwood. In the road from London there is an almost continual ascent for the last four or five miles, which makes a considerable eminence above any parts of the neighbouring country. On the highest part of it stands the church, which has at the West end a tower, and in one corner of this there is a round turret, being a continuation of the stair-case, about four feet wide, eight feet high, and the walls of it one foot thick. In the top of the wall of this turret, which was leaded, are fixed several iron bars, that are bent so as to meet in the middle and support a weather-cock, which was put up about sixteen years ago.

On Monday June 18th, 1764, between twelve and one (about three hours before the time when the thunder and lightening happened in London, by which St. Bride's Steeple and Effex-Street were damaged) there was a storm at South-Weald, attended with uncommonly loud thunder. The lightening struck the weather-cock, and passing along the iron bars, upon which it stands, rushed against the wall of the turret, and has broken a space from the top of the turret to the leads of the tower, about four feet wide, being about one third of the circumference of the turret and facing the North. The weather-cock, and irons that support it seem to be unhurt. The walls

walls of the turret were made of rough stones and mortar ; and part of what is beaten down has fallen upon the leads of the tower underneath, and part upon the roof of the church, which is greatly damaged. The stair-case also, which leads up to the turret, is so full of the stones and mortar, that it is with great difficulty and some hazard that any one can go up it. From a leaden spout at this West end of the church, which only comes down to near the top of the West window, the plaster is beaten off the wall for some inches in breadth quite to the window ; and at the bottom of the upright iron bars of this window several of the stones are cracked, and the wall is chipped here and there from thence to the ground. The same is observable in the stones at the bottom of the upright iron bars in the East window, which is also near a leaden spout that comes down from the roof over the chancel, the end of which rests upon a buttress, and does not reach the ground by several feet ; which buttress is cracked, as well as the adjoining wall. On the inside of this wall, within the church, there is a large wooden frame, which holds the commandments. This frame at the left hand corner is supported by an iron holdfast driven into the wall, which was mentioned above as being cracked on the outside under the leaden spout. The plaster of the wall, for three or four inches all round this holdfast, within the church, is beaten off ; and to the left hand there is a space, slanting from the holdfast toward the ground, five or six inches wide and three or four feet long, from which all the mortar is forced away. That part of the wooden frame, where the holdfast is fixed, is shattered. The canvas, upon which the
com-

commandments are painted, which was in this wooden frame, is torn from the frame on the two sides of it next the holdfast, and is rent besides in several places.

The whole appearance of the damage done to this church very much favours the conjecture of that sagacious observer of nature, Dr. Franklin, who thinks it probable, that, by means of metallic rods or wires reaching from the roofs to the ground, any buildings may be secured from the terrible effects of lightening.

XL. *Observations upon the Effects of Lightning, with an Account of the Apparatus proposed to prevent its Mischiefs to Buildings, more particularly to Powder Magazines; being Answers to certain Questions proposed by M. Calandrini, of Geneva, to William Watson, M. D. F. R. S.*

*To the Right Honourable the Earl of MORTON,
President of the Royal Society.*

My Lord,

Read June 28,
1764.

I Very lately received a letter from the learned and ingenious Monsieur Calandrini, of Geneva, who has a considerable employment in the Ordnance in that city. In this letter Monsieur Calandrini tells me, that he had perused with attention a letter which I wrote to the late Lord Anson, which contained some suggestions tending, as I hoped, to prevent the mischiefs occasioned by lightning to ships at sea; and which likewise might, on the same account, be useful to powder magazines. This letter was printed in the Philosophical Transactions *. He says, that he has considered with satisfaction the real advantages, which may arise from thence to fortified towns, where the quantity of gun-powder may, from any accident, endanger the whole fabric of a city.

This gentleman therefore is desirous of presenting a memorial to the Board of ordnance at Geneva; in which he would be very glad to explain to their satisfaction the method I propose. He has therefore

* Phil. Transf. Vol. LII. page 629.

sent me the following questions, which he thinks tend to throw further light upon this subject, and has desired my opinion upon them. As these may possibly hereafter be of public utility, I have taken the liberty of communicating them to your Lordship.

I. What sort of apparatus is used at Philadelphia?

II. Whether there is not some improvement to be made to their methods?

III. In what manner this apparatus may be adapted to powder magazines?

IV. Into what place the thunder may be conducted, where there is no river near, to answer the purpose of the sea about ships?

V. Whether the apparatus might not electrify the air, so as to occasion lightning, which was, he believes, the cause of the death of Professor Richmann of Petersburg? This apparatus may not be dangerous to dwelling houses, where the fire may slip without any manner of risk; but may be attended with the most dreadful consequences to a powder magazine, where the smallest spark may occasion the explosion of the whole.

VI. Whether the square, or the circular form of building, will be easiest adapted to the apparatus?

VII. Whether an iron bar fixed on the top of the building, to support a weather-cock, may not attract the thunder bolt, and be consequently dangerous to all buildings; but more especially to powder ma-

VIII. Whether there is not some particular manner of buildings, invented of late, adapted to powder magazines; either to diminish the shock of the explosion, or to secure them against any accident, by the method used at Philadelphia?

Mr.

M. Calandrini says further, that he himself has been eye-witness of the effects of lightning coming into a room, which had received much damage from it. That he looked for the place it went out at, and after long search perceived that it had followed the wire of the bell, which had conducted it through a very inconsiderable hole into the next room; from whence it had opened itself a passage into a back yard. This accident was at that time thought very extraordinary, being anterior to Dr. Franklin's experiment.

To M. Calandrini's questions I have sent the following answers.

I. The apparatus, used at Philadelphia, consists either of a long iron rod, placed upon the highest part of an house, or other building; or, of a shorter rod, inserted into a long wooden pole, placed in the same manner. The iron rod, mentioned by Mr. Kinnerfley in the Philosophical * Transactions, and which probably preserved the house in Philadelphia upon which it was placed, extended in height about nine feet and a half above a stack of chimnies, to which it was fixed; but he supposes that three or four would have been sufficient. These rods are pointed at their upper extremity. It is indifferent, which of these two are used, provided that they are of height enough to reach above the chimnies, or any other part of the edifice. Connected to, or suspended from, the metal of these, a metallic wire, generally of iron, is conducted, in the easiest and most convenient manner, to the nearest water, viz. to the well of the house, or any other water in the neighbourhood.

* Vol. LIII. page 95.

II. This method, wherever it has been employed, has hitherto perfectly answered the intention ; no house in Philadelphia, or in any other place I have heard of, having suffered from the effects of lightning, where this apparatus has been erected. The improvements I should recommend would be, first ; that, as iron wire soon becomes rusty, and when rusty to the center is unfit for the present purpose ; and as brass wire is, when long exposed to the weather, exceedingly brittle and liable to snap asunder, the wire should be of copper ; and of a size not less than that of a large goose quill. Secondly, I prefer it's being conducted, from the rod at the top to the water below, on the outside of the building, and thereby prevent the lightning from coming within the building. On houses, where there are gutters and spouts of lead to carry off the rain, the wire need only be conducted to the lead of the gutters ; and attention be had that the gutters and the spouts coming from them are in their whole length in contact, or very nearly so, one with the other. If the leaden spouts do not reach to the bottom of the building, a slip of lead, such as is employed for the gutters, and about an inch wide, should be fastened to the bottom of one or two of the spouts, and conducted to the water. If a slip of lead, such a one as has just been mentioned, was to be conducted from the rod at top to the gutters, it might with equal advantage be substituted for the copper wire : or further, a slip of lead of this kind may be connected with the rod at the top of the house ; and, where there are no leaden gutters or spouts, may be conducted on the outside of the house down to the water, as I before mentioned.

oned. I would recommend likewise an increase of their number; as the effects of one apparatus of this kind can extend only to a certain distance, and that to no great one; and the security, where mischiefs from lightning are frequent, must arise from their number. In countries and places so circumstanced, no house or other building should be without one at least; large edifices ought to have several. The number should be in proportion to the size of the building.

III. In powder magazines, I should recommend the apparatus to be detached from the building itself; and to be only placed as near it as might be. Powder magazines should never be constructed so, as to cover a large quantity of ground. If security from lightning was considered in their construction as a considerable object, I should recommend a circular building; in the periphery of which should be placed storehouses sufficient in their number and extent to contain the quantity of powder proposed. In the centre of this circle should be a well, very near which should be erected a pole or mast, high enough to reach some feet above the buildings of the powder magazine, or the buildings in it's neighbourhood. From this mast there should rise a brass rod, five or six feet in length, an inch in thickness, and ending in a point; and from this rod a wire of copper of a size not less than that of a large goose quill, should be conveyed down the mast, and terminate in the water of the well. If there is no well, the wire should be laid into the nearest water; as the expence even of some hundred yards of a wire of this sort can hardly be considered as an object in an affair of this importance.

tance. For though I have reason to believe, that the wire communicating with the ground would prevent the mischiefs of a thunder cloud, which came near an apparatus of this sort; yet as water is a more ready conductor than the ground, it should, if possible, be insisted upon in this particular case, and employed. Mr. West's apparatus, described by the before-mentioned Mr. Kinnerley, terminated in an iron stake, driven four or five feet into the ground; nevertheless the earth did not conduct the lightning so fast but that, in a thunder storm, the lightning was seen to be diffused near the stake two or three yards over the pavement, though at that time very wet with rain. It is presumed, that had this iron stake been placed in water instead of earth, the lightning had not been visible, on account of the water's receiving the electric matter more readily than earth. Where this apparatus therefore is applied to powder magazines, it should certainly terminate in water. At Mr. Hamilton's at Cobham, about twenty miles from hence, where an apparatus of this sort was erected upon an high and greatly-exposed building, as there was no water but at a great distance, the bottom of the wire was placed deep in an hill of moist sand. If, instead of one wire, two, three, or more, were adapted to the brass rod in this manner, and conducted to the water, or if the brass rod itself was continued to the water, I should consider it, in extraordinary cases, as an additional security. This will explain my sentiments upon the third, fourth, and sixth questions.

V. As the expectation of the utility of this apparatus is presumed to be the preventing of the accumulation of electricity in its neighbourhood, by afford-
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ing a constant and easy passage to the electricity of the clouds furcharged therewith, nothing, in my opinion, need be apprehended from the apparatus electrifying the air ; as its principal operation is conceived to be the reverse of that, viz. divesting the air of it's electricity. I am well apprized from experiments made here, that the earth is frequently electrified *plus*, and the clouds *minus* ; and that this change of *plus* and *minus* between the clouds and earth are sometimes seen to vary several times in a quarter of an hour : but in that case it is presumed, that the clouds, within the sphere of action of the apparatus, have by it's operation their electricity brought to the same standard with that of the earth in its neighbourhood, and *vice versa* ; and consequently, that the mischiefs which might arise from the difference of the densities of the electricity in the earth and clouds are prevented, by the equilibrium between them being maintained. This subject, in relation to the electricity's being *plus* or *minus*, I many years ago considered, and laid my thoughts thereupon before the public, as may be seen in the Philosophical Transactions, Vol. XLV.

That the atmosphere at times is very strongly electrified, is evident, to say nothing of lightning, not only from our apparatus, but from the masts of ships, being beset with *St. Elmo's fires*, which I believe would scarce, if ever, happen, were the masts provided with an apparatus of this sort ; unless the cause might be so great, and come on so fast, that the metal employed between the tops of the masts and the water might, not, on account of the vastness of the cause, be large enough for the purpose. If it should so happen, *St. Elmo's fires* might still appear
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at the tops of the masts, and thunder clouds might burst near them, and exert their dreadful effects *. That even artificial electricity, when in too great a quantity, and hurried on too fast through a fine iron wire has a remarkable effect upon the wire, appears from a very curious experiment of Mr. Kinneresley of Pennsylvania. This gentleman in the presence of Dr. Franklin, by *his case of bottles* being electrified fully, and made to explode at once, after the manner of the experiment of Leyden, through a fine iron wire, the wire appeared at first red hot, and then fell into drops, which burned themselves into the surface of his table or floor. These drops cool in a spherical figure, like very small shot, of which Dr. Franklin transmitted some hither to Mr. Canton †, who has repeated this experiment. This proves the fusion to have been very compleat, as nothing less than the most perfect fluidity could give this figure to melted iron. These effects from artificial lightning, are exactly similar to those of the natural; as we have several times known iron wires,

* See more upon this subject Phil. Transf. Vol. XLVIII. page 215.

† The diameter of a piece of Mr. Kinneresley's wire, which I received from Doctor Franklin, was one part in 182 of an inch. Artificial lightning from a case of 35 bottles, I find will entirely destroy brass wire of one part in 330 of an inch. At the time of the stroke, a great number of sparks, like those from a flint and steel, fly upwards, and laterally from the place where the wire was laid, and lose their light in the daytime at the distance of about two or three inches. After the explosion, a mark appears on the table the whole length of the wire; and some very small round particles of brass may be discovered, by a magnifier, near the mark; but no part of the wire itself can be found.

J. CANTON.
nails,

nails, and other metallic substances to have been melted, and parts of them, while hot, bedding themselves in wood, by a thunder storm. Of this we had some instances here in a thunder storm, which happened in July 1759, of which the effects were communicated to the public in the * Philosophical Transactions. As metal has been made red hot, and melted by artificial lightning, how much greater must be presumed to be the effects of the natural; and how much larger ought to be the metallic part of the apparatus, to avert its mischief? This requires particular attention.

VII. I was of opinion, that iron bars to support weather cocks, if they were placed upon the tops of buildings made of brick or stone, and in contact with either of these materials, were not dangerous to ordinary buildings on the account you mention, except in very particular and extraordinary cases; as these substances, when not much heated, conduct the electric matter in a very considerable degree. But what lately happened to St. Bride's Steeple, as well as the mischief to South-Weald church on the same day, evinces, to me at least, that the apparatus, usually applied to weather cocks, should never be trusted in any building, without a metallic communication from them to some water, or at least very moist ground. St. Bride's Steeple, one of the most beautiful in London, was, on Monday, June 18, about ten minutes before three in the afternoon, very greatly injured, in one of the most severe thunder storms, which ever happened here.

From as attentive an examination, as the steeple at the present will admit of without scaffolding, it

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appears

appears to me, that the weather-cock and its apparatus had the principal share in occasioning the great mischief done to the upper part of the steeple. I am of opinion, that the lightning first took the weather-cock and was conducted, without injuring the metal or any thing else, as low as where the large iron bar or spindle, which is inserted into the top of the steeple, and comes down several feet of its length, terminates. There the metallic communication ceasing, part of the lightning exploded, cracked and shattered the obelisk, which terminates the spire of the steeple, in its whole diameter, and threw off at this place several large peices of Portland stone, of which this steeple is built. Here it likewise removed a stone from its place, but not far enough to be thrown down. From hence the lightning seems to have rushed upon two horizontal iron bars, which are placed within the building, cross each other, to give additional strength to the obelisk, almost at the base thereof, and not much above the upper story : here, on the North East and East side, it exploded again at the end of the iron bar, and threw off a considerable quantity of stone. And here, for the sake of explanation, I must observe, that the spire of this steeple, where it rises above the bell tower, is composed of four stories, besides the obelisk placed over them. The lowest and second are of the Tuscan order ; the third is Ionic ; and the fourth or uppermost Composite or Roman. The stone piers of these stories are connected together and strengthened by iron bars placed horizontally near the height of the capitals of the pilasters, and each story has only one set of these bars. From the cross bars near the base
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of the obelisk just now mentioned, the lightning broke through the roof above the Composite story; at the ends of another set of iron bars placed lower than the former, from which it tore out a large portion of the stone. It then struck the iron bars of this story, which are placed immediately under, and in contact with the stones, broke one of the iron bars directly across, and bent the larger part of it from its horizontal direction to near an angle of 45° . Its rapid progress being here in some measure prevented, at the end of one of the iron bars, it threw off the upper part of one of the Composite pillars just above its capital and a large portion of the cornice projecting over it, and that with such a force, that part of a stone which was placed here and formed a portion of the cornice, and weighed seventy two pounds, was projected, not only the whole length of the body of the church, but beyond it, across St. Bride's Lane; where it fell upon the top of an house, and broke through the roof and lodged in the garret. The horizontal distance from the steeple to the place where it fell, was at least 150 feet; the height, from which it fell, somewhat more than two hundred. This piece of stone was of a very irregular figure, and must have required an amazing force to rend it, detach it from the building, and throw it to such a distance. The shaft of the pillar, the next to the East of that whose upper part had suffered so much, was likewise violently struck; and a large portion of its diameter broke out and thrown down. The Ionic story has suffered considerably, more particularly the pilaster fronting the North East, and placed directly under the Composite column, whose top was

thrown off. This pilaster is much injured, but the story in general has suffered less than the Composite, and that chiefly where the irons are inserted; the upper Tuscan less than that, and the lower Tuscan but little, except in the North East pier, which is considerably cracked and shaken; as if in its passage part of the force of the lightning was spent in these explosions, and part absorbed and conducted by the masses of stone. The damage done to the steeple is, except near the top, confined almost to the East and North East side, and most generally where the ends of the iron bars have been inserted into the stone or placed under it; and in some places, by its violence in the stone, its passage may be traced from one iron bar to another. And it is very remarkable, that, to lessen the quantity of stone in this beautiful steeple, in several parts, cramps of iron have been employed; and upon these, stones of no great thickness have been placed, both by way of ornament and to cover the cramped joint. In several places, these square stones have, on account of their covering the iron, been quite blown off, and thrown away. A great number of stones, some of them large ones, were thrown from the steeple, three of which fell upon the roof of the church, and did great damage to it; and one of these broke through the large timbers, which form it, and lodged in the gallery.

In the tower of the steeple, in the room where the bells are placed, the lightning took the South-west window above the bells and close to the window, not far from an iron bar, which goes round, and rent out several large stones; some of which fell into the bell, which was very near this part of the steeple, and

and was the largest in the steeple; and, passing below the bell, tore out at another place, in a line with the former, a great number more. One of the stones, torn out above the bell, was thrown to the North-east side of the tower. Between the two places, in which the lightning had here exerted its fury, the wooden block, which confined the axis of the frame of the great bell, and was fastened down with two iron staples, was thrown off, and the staples torn out. No damage at present seems done to the bell.

It is remarkable, that, less than twenty years ago, one of the stones of the obelisk of this steeple was observed to be moved from its place, and project some inches over those under it. This stone was about seven feet from the top of the obelisk. Danger being apprehended from this state of the spire, it was taken down to the place where the stone was removed, and rebuilt with new stone. This accident, at that time, was supposed to be owing to the ringing of the bells; but it is highly probable, from what has lately happened, that, as that stone was removed from its place, very near to that part of the spire, where it is now cracked and shivered quite a cross and several pieces of stone thrown down, it was owing to the same cause as the present damage, viz. lightning, though not at that time adverted to.

The lightning on June 18 came from the West and South-west; the damage done both to St. Bride's church and South Weald was on the East and North East sides, except that in the bell-loft at St. Bride's. The stones both from the steeple of St. Bride's and in its tower were thrown to the East and North East.

Since

Since the communication of this paper to the Royal Society, the steeple of St. Bride's has been surveyed, and found so very much damaged in several of its parts, that eighty five feet have been taken down, in order to restore it substantially. Within these eighty five feet are comprehended the obelisk, placed at the top of the steeple, the small dome immediately under it, the space between that and the uppermost or Composite story, the Composite story, and the Ionic story. This last, on the East and North sides, was taken down to its bottom; but on the other sides, as they were not injured, some parts were permitted to stand. Three piers were likewise taken down of the second Tuscan story, and one pier of the first. The scaffolding to take this down and rebuild it enabled me minutely to examine, not only the damages occasioned by the lightning, but the manner of its progress. This examination confirmed the opinion of the cause and manner of this accident, which I communicated to the Royal Society, soon after it happened; and before a near inspection could be obtained. It completely indicated the great danger of insulated masses of metal to buildings from lightning; and, on the contrary, evinced the utility and importance of masses of metal continued, and properly conducted, in defending them from its direful effects. The iron and lead employed in this steeple in order to strengthen and preserve it, did almost occasion its destruction: though after it was struck by the lightning, had it not been for these materials keeping the remaining parts together, a great part of the steeple must have fallen.

The operation and progress of the lightning in the obelisk and upper parts of the steeple deserve more particular attention. To form a more perfect idea of these, the following measures will in some degree contribute.

	Feet	Inches.
The height of the octogonal obelisk - - - - -	22	- - 3
Length of the iron spindle - -	19	- - 9
Thickness of the spindle, where inserted into the stone. - - -	0	- - 2 square
Its length inserted into the stone	9	- - 10
From the bottom of the spindle to the first cramped joints. - -	5	- - 10
Three courses of stones without cramps. - - - - -	5	- - 7
From the bottom of the spindle to the first concealed chain. - -	11	- - 5
From the first concealed chain one foot above the base of the obelisk to the first cross chain. - -	2	- - 0
From the first cross chain to the second, placed in the dome -	8	- - 10

The vane, the cross above it, the ball and its socket, which covered so much of the spindle as arose above the stone, to near ten feet of its length, were of copper gilt. This length of the spindle was cylindrical, but the other part was made square, where it began to be inserted into the stone. To fasten this spindle more securely in the courses of stone, melted lead had been poured. This lead, in the two lower courses of stone through which the spindle had passed, not only filled all the space left between the spindle and the stones,

stones ; but had, as it were, ramified itself not only between the joints of the stones, but had insinuated itself in its melted state into all their small clefts and interstices. The spindle terminated in one stone, which occupied the whole area of the obelisk, and was three feet and near two inches in diameter, and one foot in thickness. Into this stone the spindle was inserted five inches of its depth, and fastened by melted lead. Under this stone the obelisk was hollow ; but above it was solid, excepting the space left for the spindle.

Upon examining these several particulars, no injury had been done by the lightening to the vane, its cross, copper ball, or spindle. Of the seven courses of stone at the upper part of the obelisk, and which were above the whole stone into which the spindle was inserted, the five upper courses, though connected together at top and bottom with iron collars sodered with lead, were not damaged ; but the two stones, which formed the sixth course, were cracked, shivered, and fragments thrown from them. The seventh course consisted likewise of two solid stones. These were burst from the spindle, which was, by the intervention of the lead, connected with them, broke into many parts ; each was moved from its place ; some pieces were thrown down, and one large one projected five inches over the stone, immediately under it. The whole stone, into which the spindle was inserted, and upon which it rested, was burst from the center into a great many pieces, and every piece removed from its place. Some of these were thrown from the steeple. Several of the larger masses of this stone, which still cohered, were very much shivered. The center of the stone,
upon

and near which the spindle rested, was beaten to powder, and a hole made through the under part of the stone. That this stone in this condition should still support the seven courses above it, which weighed four tons, exclusive of the spindle, vane, and their appertinances; and that the whole did not fall when struck with the lightning, is in no small degree surprising.

From the bottom of the spindle to the first course of stone, where the workmen had used iron cramps, the distance was five feet seven inches. These cramps were bedded in the stone. Part of the lightning, from the bottom of the spindle through the hole just now mentioned, seized these cramps, and threw off large scales of stones at their ends. From these there were three courses of stone, in which there were no cramps; these suffered nothing.

In edifices of this kind, for additional strength, the builders employ bars of iron, connected together in such a manner as their exigencies require; and these, though they have no links, are denominated chains. These are sometimes so adapted to the courses of stone as not to be visible, and are perfectly concealed: at other times, they are in part visible, and in part concealed.

The first metal, that occurred after the cramps before mentioned, was a concealed chain, one foot above the base of the obelisk, and two feet above the first cross chain. Here two stones were burst and shattered. In the course of stone, where the first cross chain was inserted, and the several stones connected by iron cramps, many of the stones were much shattered.

At the base of the dome, near nine feet below the first cross chain, was a second. This chain was a double cross connected at its ends with a circle of iron, which was bedded into the whole course and fastened by melted lead. Here the lightning made great ravage, burst and threw off the stones in which the iron circle was bedded, and tore out part of the roof of the dome, threw off two pieces of the cornice and one of the vases, which was contiguous to it. These two pieces of cornice weighed twelve hundred pounds. The courses of stone between the two chains, except those I just now mentioned, were not injured.

To what is here said, I shall only add, that in no part the steeple was injured, except where the stones were in contact or very near the iron and lead employed in its building; and the quantity of stone burst, spoiled, or so much damaged as not fit to be used again, amounts, as I am informed by Mr. Stanes, a very honest and ingenious mason, who has contracted to repair the damage done by the lightning, to not less than five and twenty tons. An amazing quantity!

The above mentioned Mr. Stanes was employed, a few years since, in the repair of the steeple of St. Mary le Bow in Cheapside, which was injured by a very rare and uncommon accident. At its erection, the builders had employed, near the top of the spire, for additional security, several iron cramps; the ends of which, by being exposed to the weather, became rusty, swelled, and so much enlarged thereby, as to raise the stones above them, and to deflect the top of the spire six inches from the perpendicular. Danger being apprehended from this situation, the spire was
taken

taken down several feet of its length, and properly repaired. This ought to be a caution to succeeding builders, that if, in edifices of this kind, they find it expedient to employ cramps, they should be either of copper, which is not liable to swell by moisture; or, if iron be used, so much space should be left in the under bed of the stones, which immediately cover the cramps, that they may have room to extend themselves without danger to the building. This remark, though not immediately relating to our present purpose, will not, I hope, be thought impertinent in this place.

But to return: this thunder storm had been preceded by several very warm days. The nights had scarce furnished any dew: the air was quite dry, and in a state perfectly unfit to part with its highly-accumulated electricity without violent efforts. This great dryness made the stones of St. Bride's steeple, and all other buildings under the like circumstances, far less fit than if they had been in a moist state, to conduct the lightning, and prevent the mischief. For though this thunder storm ended in a heavy shower of rain, none except a few very large drops fell till after the church was struck; and I have no doubt, but that the succeeding rain prevented many accidents of a similar kind, by bringing down with every drop of it part of the electric matter; and thereby restoring the equilibrium between the earth and clouds. It is frequently taken notice of, in attending to the apparatus for observing the electricity of the clouds, that tho' the sky is much darkened, and there have been several claps of thunder at no great distance, yet the apparatus will be scarce affected by it; but as soon

as the rain begins, and falls upon so much of the apparatus, as is placed in the open air, the bells of the apparatus in the house ring, and the electrical traps succeed each other in a very extraordinary manner. This demonstrates, that every drop of rain brings down part of the electric matter of a thunder cloud, and dissipates it in the earth and water; and prevents thereby the mischiefs of its violent and sudden explosion. Hence, when the heavens have a menacing appearance, a shower of rain is much to be wished for.

From these considerations, I have no doubt, but that the mischief done to St. Bride's steeple was owing to the efforts of the lightning, after it had possessed the apparatus of the weathercock, endeavouring to force itself a passage from thence to the iron work, employed in the steeple. As this must be done *per saltum*, there being no regular metallic communication, it is no wonder, when its force is vehement, that it rends every thing which is not metallic, that obstructs its easy passage; and in this particular instance, the ravages increased as the lightning to a certain distance came down the steeple. To procure this easy passage and avert the rage occasioned by the want of it, in future, as much as our present knowledge in these matters will enable us to do, I cannot sufficiently recommend metallic communications between the metal at the top and water, either as has been before mentioned, or in any other convenient manner, taking care not to be too frugal of the metal employed. This was first suggested by that excellent Philosopher Dr. Franklin; and since much used in Philadelphia, and other parts of North America.

Near

Near the same time, that the mischief was done to St. Bride's church, the mast of his Majesty's ship Ramillies, lying at Chatham, was split and torn to pieces by the lightening. This is the less extraordinary, as, from its height, figure, and constituent parts, the mast of a ship stops the progress of lightening much more than edifices of the same height, made of brick or stone. This therefore seems to require particular attention; but upon this head I fully explained myself in my letter to the late Lord Anson *, and shall therefore decline saying any thing further of it in this place.

I flatter myself, that what has here lately happened will tend to occasion the applying of an apparatus of this sort to all buildings, at least, of value and extent. No steeple should certainly be without it; and in most, if the iron work of the weather-cock can be easily got at, it may be adapted with very little trouble or expence. It is only necessary to make a metallic communication between this iron work and the lead, which carries off the water. This frequently reaches to the ground or very near it. From the bottom of this, the metallic communication should continue to the nearest water, or at least to very moist ground; though where it can be procured, water should be preferred. Care must be taken likewise, that metallic communications be added to such parts of the lead, which serve to convey the water from the top, as do not already touch or come near each other. And these may be either of lead, or of copper wire, such as I have before mentioned. In thunder storms attended with rain, sufficient in quan-

* Philosophical Transactions, Vol. LII. p. 629.

tity to run off in streams, a great portion of the electric matter runs off in, and is dissipated by, these streams; and buildings are thereby preserved from damage.

What happened to St. Bride's cannot but give us some apprehensions for that most noble edifice in its neighbourhood; I mean St. Paul's. This is above an hundred foot higher than St. Bride's, and therefore more in the way of accident from thunder storms. Upon its magnificent lanthorn is placed a cross of metal, which is inserted into the stone of the lanthorn; and this is supported by a truncated cone of brickwork, which arises from the arches of stone below. The cupola is covered with lead, which is continued to the spouts of the same material. These bring down the water to the stone gallery under the cupola, and end within about a foot of the stone. From hence the water is conveyed a considerable distance, in a stone trough or channel, to the leaden spouts; and these are carried down the building, and terminate, as I was informed upon inquiry, in the common sewer. By this arrangement the metallic communication is interrupted. In thunder storms during rain, the water carries off in its streams the electricity, as perfectly, as the most compleat metallic communication would; but when there is no rain, it is otherwise; and these interruptions are the great cause of danger. To lessen which, as far as we can conclude at present, it would be expedient to make, by the means of several copper wires, small rods, or pieces of lead, a metallic communication between the gilded cross, and the lead of the cupola: and again, from the leaden spouts of the stone
gallery

gallery to those, which bring the water thence; care being taken that, from the bottom of these last, there should be a metallic communication, if there should be found to be none at present, with the water in the common sewer. Thus, without much expence, a compleat metallic communication may be made between the top of St. Paul's church, and the water; which had it been done at St. Bride's, the ravages so lately experienced had in all probability been prevented.

From considering the circumstances of this thunder storm, I cannot but be of opinion, that the injury done to St. Bride's prevented mischief to St. Paul's. St. Bride's is a very high building, and within a small distance nearly West of St. Paul's. When this distance is considered, and that the lightning came in the direction of St. Bride's to St. Paul's, and that when the thunder cloud came near the former, it exploded there, and parted with much of its force; what was left did no damage to the latter, tho' the much higher and more exposed building, and having a metallic cross at its top.

I have recommended as metallic conductors copper wires of the size of a goose quill; as, when of that thickness, they may easily be bent to any direction; and, where thought necessary, any number may be employed. I look upon this as a kind of standard, from what Dr. Franklin wrote to Monsr. Dalibard of Paris upon this subject *. He observes, in a church which suffered greatly by lightning at Newbury in New-England, that though a small wire was beaten to pieces by lightning, and dissipated by its force, the rod of a pendulum conducted the whole without

* See Phil. Trans. Vol. XLIX. p. 305.

being

being melted or otherwise injured by it; and that, great as the quantity was in this instance, and which utterly destroyed the small wire, no damage was done to the building, as far as the small wire, and the pendulum of the clock extended: and in the remarkable instance, mentioned by Mr. Kinnersley in his * letter to Dr. Franklin, where a brass wire of about two lines thick, ten inches long, and terminating in a very acute point, was inserted into the iron rod, about two inches and half only of its top were melted by the lightning; the remaining part of it transmitting the lightning without being fused by it.

You will observe in this disquisition, that I have no where mentioned the apparatus attracting the lightning. I have avoided introducing the term *attraction* here, operating as an active principle; as I consider the apparatus purely passive, and only affording, from the aptness of its parts to that purpose, an easy and uninterrupted passage to the lightning, and thereby preventing its violent efforts.

You will pardon, Sir, this long digression in relation to St. Bride's church; as it gives so positive and explicit an answer to part of your seventh question; such a one as could not, without the late thunder storm, have been furnished, at least from hence: To wit, that, without a proper apparatus, weather-cocks placed at the tops of any buildings are dangerous to them in thunder storms; but more especially to powder magazines.

The accidents, which have lately happened to St. Bride's and South Weald churches, if considered as great electrical experiments, furnish very important, and, I flatter myself, useful conclusions. They are too hazardous

* Philosophical Transactions, Vol. LIII. p. 96.

and expensive however, to wish to see often repeated.

If the erecting of an apparatus of this sort should become general in countries where thunder storms are frequent and often attended with mischief, though damage should really be averted by it, the operation of the apparatus would be unseen, and therefore unknown, unless in such rare instances as that mentioned by Mr. Kinnerly. To make its effects apparent, as has been hinted to me by Dr. Heberden, a very deservedly eminent physician here, if chains are employed as metallic communications, instead of wires or rods, whenever the lightning comes near enough to affect the apparatus in a considerable degree, it will without mischief be visible in the dark, by its sparkling and snapping in its passage, at the links of the chain.

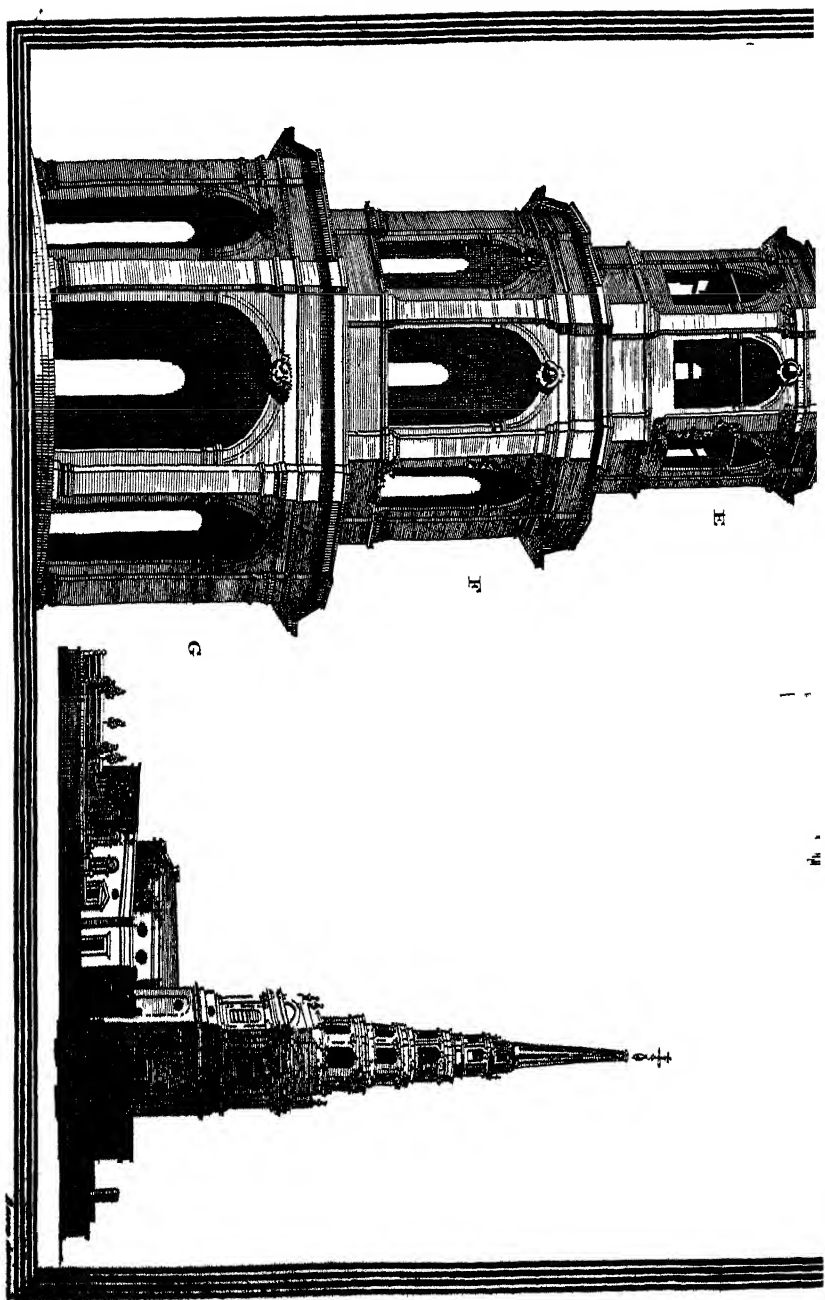
The effects of the apparatus may be observed in another manner. If the metallic communications are by the means of a wire or single rod, there may be, in some part of its length, in any place convenient for observation, a space left where the metal is discontinued; but this space should not exceed two inches. The two extremities of the metal at this interruption should be furnished with brass knobs not less than an inch in diameter. By this method, though the effects of the apparatus would not be considerably lessened, they might be observed. For at times, when no lightning was visible, but when clouds replete with it came near the apparatus, or rain from them fell upon it, there would be a snapping from one of the brass knobs to the other. When indeed the lightning was near, there would not only be this snapping; but, if the cause was great, a stream of fire would be

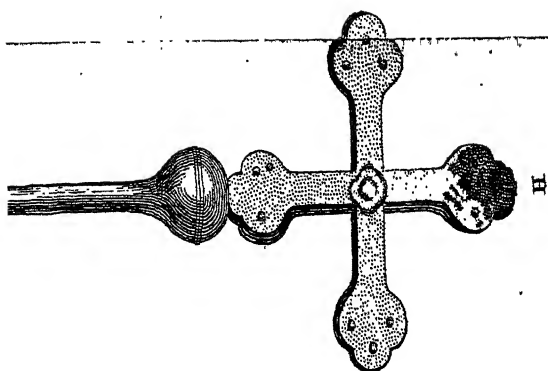
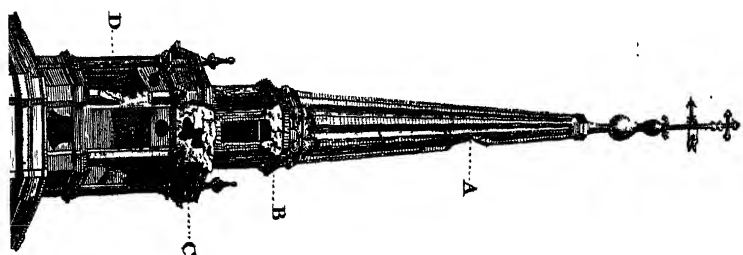
seen, as in M. Romas's kite*, to pass from one of these to the other, as the best and nearest conductor. If danger however is apprehended, a piece of chain may be always at hand to be hung occasionally upon the upper knob, so as readily to fall in contact with the lower. Otherwise, if while the metallic communication is divided, though when entire it is apprehended it may be touched with safety, a person should touch the rod above the division and at the same time touch or come very near the rod below the division with any part of his body; and at the same instant if a smart stroke of lightning affected the apparatus, he would certainly be destroyed, as happened to professor Richmann at Petersburg; the lightning going through his body from one part of the apparatus to the other, which it is believed it will not do, while the metallic communication is complete.

VIII. I have not heard that there has been here of late any particular mode of buildings, adapted to powder magazines, to diminish the shock of the explosion in case of accident: nor do I believe that any attention has been here given, in constructing these buildings, to prevent, by an apparatus of this kind, the effects of lightning.

These, my Lord, are my answers to M. Calandrini's questions. If they are satisfactory to that ingenious gentleman, or have the least tendency to public utility, I shall be gratified. As I know your Lordship's zeal for philosophical discussions, I have taken the liberty of sending you these queries and

* Philosophical Transactions, Vol. LII. p. 341.





my answers to them, as a testimony of the very great esteem and regard, with which I am,

My Lord,

Your Lordship's

most obedient,

humble servant,

Lincoln's Inn-Fields,
June 26th, 1764.

W. Watson.

XLI. *An Account of the Effects of Lightning in St. Bride's Church, Fleet-street, on the 18th of June 1764: In a Letter to Mr. Benjamin Wilson, F. R. S. from Edward Delaval Esq; F. R. S.*

S I R,

Read June 28, 1764. **T**HE inclosed is an account of the effects of the lightning on the steeple and spire of St. Bride's church, with drawings [TAB. XIV. XV.] which very accurately express the parts damaged by it.

I thought it would be of use, by describing the several circumstances of this accident, to shew more fully

fully the necessity of preventing the danger, that such buildings are exposed to.

The construction of the spire is somewhat similar to that of an apparatus purposely contrived to draw the lightning from the clouds, as it runs up towards a point, and ends in a metal vane and cross, the figure of which, as well as the materials that they consist of, seem calculated to admit the lightning with the least resistance.

At this place the first marks of it are seen: at H the top of the copper cross, which is the highest part of the building, the gilding is by the explosion partly torn off and partly discoloured, so as to differ remarkably from the rest of the cross where the gilding is very well preserved. Some small pieces of solder are melted; and all this part appears as if it had been exposed to the fire.

The lightning seems to have entered here, and to have been conducted as low as the hole at A, by an iron spindle twenty feet in length, and two inches in diameter; of which ten feet were surrounded by the copper ball, vane and cross; and the lower half was inclosed in a groove cut through the middle of the solid stones which composed the upper part of the spire, and rested at A on the bottom of that groove, which was sunk five inches deep into the lowest of those solid stones: this last mentioned stone being three feet broad and one deep. The interval between the sides of the spindle and the groove made to receive it was filled up by melted lead poured in between them.

The lightning accumulated in the metal, having its passage towards the earth strongly resisted at this place, has in expanding itself formed the hole A, by
burst-

bursting off from the lower part of the spindle the stones contiguous to it on that side.

At each of the angles of the metal, the stone on which it rested is cracked, which probably was occasioned by the lightning issuing with greater freedom from those parts, than from the flat surface.

No part of the spindle is in the least injured by the lightning, notwithstanding the great quantity which, from it's effects, appears to have been accumulated in it *.

From hence, as low as to the corniche B, it seems to have been conducted along the surface of the spire, which was wetted by the rain that had fallen in the morning, before the lightning: and having been accumulated in the iron bars B and C, in discharging itself from them, it has made the greatest explosion at this place.

Under this part the freedom of it's passage seems to have been hindered by all the dry stonework underneath, which was defended from the rain by the corniches:—and it appears from some experiments which I formerly made †, that dry free stone, when warm'd to a certain degree (which probably does not exceed the heat which the stones of buildings acquire in hot weather) resists the passage of the electric fluid or lightning so strongly, that with plates of that stone, instead of glass, I performed the Leyden experiment.

* In the year 1750 the stones surrounding this spindle were so much damaged, that there was a necessity of taking them down and rebuilding that part of the spire. The cause of this was not known at that time, it is probable that it was occasioned in the same manner as the present accident.

† Philosophical Transactions, anno 1759. p. 83.

Under

Under the cornice, the lightning descended only by leaping from one iron to another; and at every leap its force seems to have been weakened, and at last to have been quite dissipated.

On examining the inside of the steeple beginning from the top, the first effect of the lightning that appears is a hole in the stone work at B, beginning immediately above an iron bar which served to support the top of the window or opening, and running upwards towards the two cross iron bars: this, when viewed from the outside of the church, is seen to have spread round most of the lower part of the spire, so that it seems in great danger of falling.

The next stroke is about four feet below: at this place four iron bars lie horizontally across the spire, and are tied together by chain bars which are inclosed in the stonework: where the end of one of the cross bars is inserted in the stone, the lightning has burst open the hole described at C, and, when the same is viewed at the outside, a great part of the cornice appears to be broken off.

At D, where the two iron bars serving to support the top of the windows meet and are joined together, the lightning accumulated in them has broken off the pier by which they were inclosed.

At Fig 2, a bar of iron, which served to support the top of the window in the same manner as those last mentioned, 21 inches long clear of the stonework, and half an inch thick, is broke and bent into the position expressed in the drawing; and the stones immediately above it are shattered and disjointed.

The sills of two windows of this story are torn off from iron bars which lay beneath them.

At

At E, an iron bar N°. 1. about twenty five inches long, was inclosed nine inches deep in the stonework of the pier, separating the East arch from the arch next it towards the North: the end of this bar joins at a right angle another bar, N°. 2, which is laid across the arch. The lightning accumulated in the iron (N°. 1.) which was inclosed in the stonework, has burst off all the stone that surrounded it, and part of the pier adjoining. The flaw is continued downwards, as expressed in the drawing, meeting with smaller iron cramps in it's way.

At F, the next arch lying immediately under the last mentioned one, an iron was inclosed in the stone in the same manner as the bar at N°, 1. The stone is torn off from this iron exactly in the same manner as at N°. 1: but the damage has not reached much further than the stone which was contiguous to and covered this bar. At the bottom of this arch the fill stone, which covered some cramps of iron, is torn off from it's place.

At G, the next arch under this, the force of the lightning seems to have been much diminished, a small part of one stone only being broken.

From the wall at the West side of the South window of the belfry some stones are thrown down: one chalky stone in particular is reduced into an impalpable powder, and the wall under the West window is almost covered with the powder: this stroke seems to have been directed towards the bells one of which is very near the place damaged: the bells have not been examined; nor can they, as I am informed, without danger of shaking the spire by their motion.

This

This is the lowest mark which is left of the effects of the lightning.

In every part that is damaged, the lightning has acted as an elastic fluid, endeavouring to expand itself where it was accumulated in the metal: and the effects are exactly similar to those which would have been produced by gun-powder pent up in the same places, and exploded. Amongst many other stones thrown to a considerable distance by these explosions, one weighing above seventy pounds was removed fifty yards Eastward from the steeple, where it fell through the roof of a house.

It is evident that these effects would have been prevented, if a sufficiently large metallic conductor had been extended from the metal at the top of the spire down to the earth, communicating with the other metallic parts of the building that lay in its way.

From several observations which I made on this occasion, such a communication seems necessary in buildings of this form. The iron bars, which were fixed in the stonework of the East arches were struck by the lightning, while those in the arches fronting them on the West side of the same story remained untouched by it. So that I do not apprehend, that a conductor communicating with the West arches only, would have preserved the opposite ones from the damage which they have suffered.

When such buildings are exposed to very large clouds replete with lightning, there is no reason to imagine that they will not convey some of their contents to other metallic parts of the building at the same time as to the metal at the top: for though
the

the conductor may be large enough to convey to the ground, from the top, all the lightning that enters that part; yet one such small conductor cannot be supposed to exhaust those immense bodies so quickly, as to disable them from striking at the same time other buildings, or other parts of the same building.

A wire, or very small rod of metal, does not seem to be a canal sufficiently large to conduct so great a quantity of lightning to the earth; especially when any part of it, or of the metal communicating with it, is enclosed in the stone work: in which case, the application of it would tend to increase its bad effects, by conducting it to parts of the building which it might otherwise not have reached.

Dr. Franklyn, from observing that the filleting of gold leaf on the cover of a book conducted the charge of five large jars, reasons that a wire will be sufficient to conduct the lightning from the highest buildings to the earth.

But it appears from an experiment of his own, that a much larger body of metal, when inclosed between small plates of thick looking glass, is not sufficient to conduct a fifth part of such a charge, without being melted, and bursting to pieces the plates of glass.

And it is remarkable, that in those parts of the church where the effects of the lightning are most conspicuous, the iron was inclosed in a resisting substance similar to the glass surrounding the gold leaf in that experiment.

Wires, instead of conducting the lightning, have frequently been melted by the explosion. So that,

I think, a conductor of metal less than six or eight inches in breadth, and a quarter of an inch in thickness (or an equal quantity of metal in any other form that may be found more convenient) cannot with safety be depended on, where buildings are exposed to the reception of so great a quantity of lightning. These are the only points in which I have ventured to differ from Dr. Franklyn.

I shall not think my time ill employed, if these observations contribute to our security against the effects of lightning. Certainly an inquiry into the properties of the electric fluid, as it furnishes us with the means of preventing such accidents, is far from being an useless speculation. I am,

Sir,

your most humble servant,

Old Palace Yard,
June 28, 1764.

Edward Delaval.

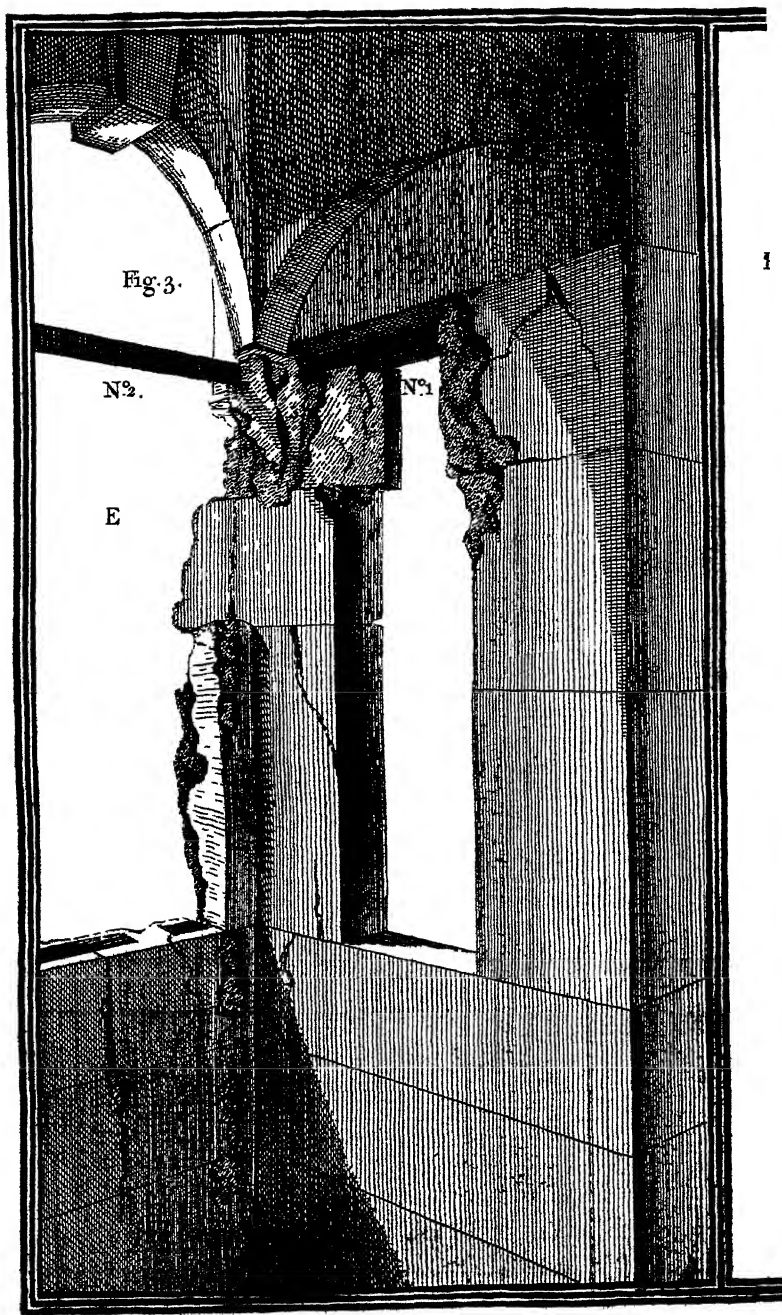
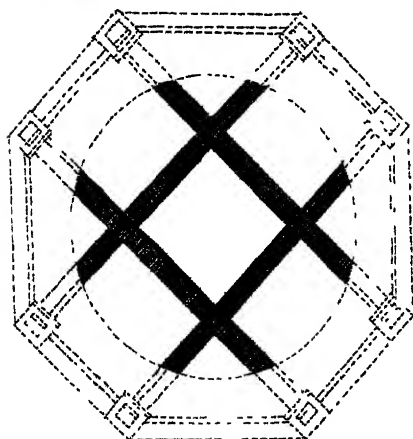
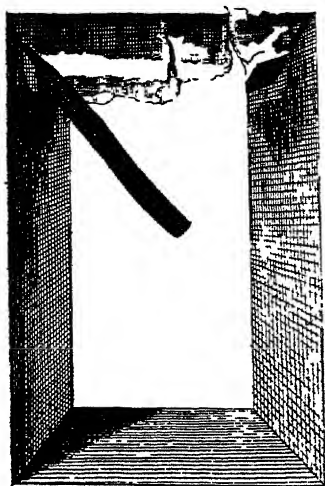
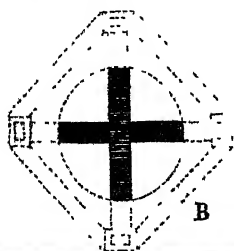


Fig 2.

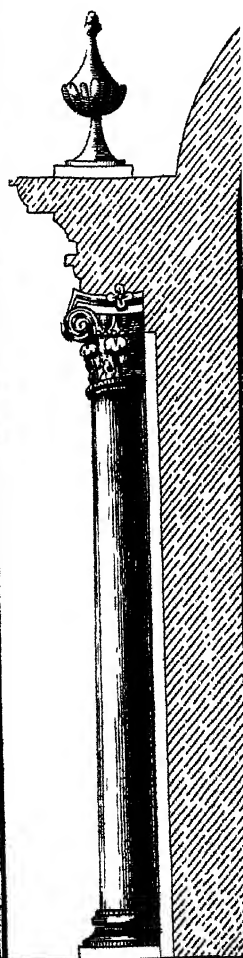


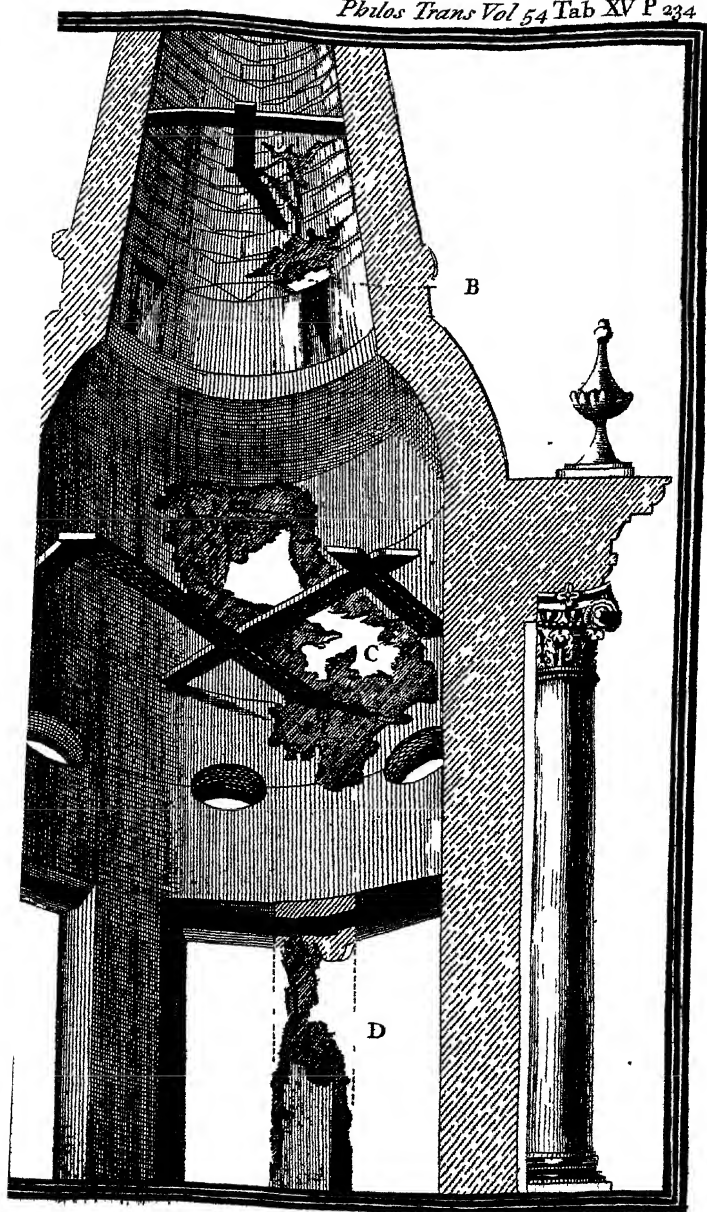
C



B

Fig1.





XLII. *A Letter from Thomas Lawrence, M. D. to William H. berden, M. D. and F. R. S. concerning the Effects of Lightning, in Essex-street, on the 18th of June, 1764.*

S I R,

Read July 5, 1764. **I** Send you, as you desired, an account of the effects the lightning on Monday se'nnight had in my neighbourhood. The storm, which came from the South-east, broke first on the two houses at the bottom of Essex-street (which look from their south windows upon the river) and beat down several feet of the east-flue of the chimney on the west side, and separated the remainder down to the roof of the house from the western flue by a wide crack. From hence the lightning went higher up the street, and at the distance of about eighteen yards from the chimney just mentioned, went thro' the eves of a house, in a direction from the North-east to the Southwest, as appeared by the breach, and forced the cieling of the garret inward by a kind of pointed bulge, without breaking the laths. It continued up the street, perhaps along the leaden gutter, over the eves of the houses for thirty yards, as ' gueis, and turned downward by the side of a leaden pipe made to convey the water from the top of the house, and tore a wooden case at the lower end of that pipe, cracked the wall near that place, and broke several

H h 2

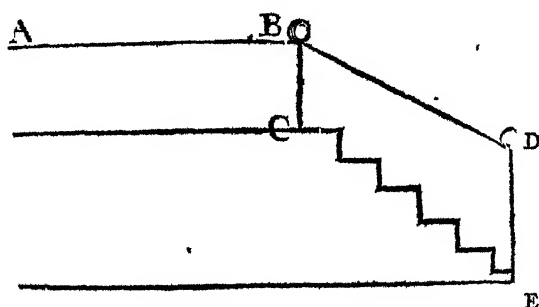
panes

panes of glass in the kitchen window next it. The wall that was cracked was blackened, and there was a strong smell of sulphur in the street.

On the east side of the street, the lightning broke the south garret window of the bottom house, threw down the eastern flue of the chimney down to the roof of the house, and took away part of the western flue. The lightning seemed to have passed between the garret window and the chimney, as the window was damaged on the west side; but the chimney, which stood west of the window, on the east side.

The tiles on the roof of both houses were broken both on the south and north side in a deep furrow, as if a heavy plough had passed over them.

The house last mentioned has a door on the east side, which opens into a garden looking into the Temple; from this door there are several stone steps down to the garden. On the left hand of the steps is an iron rail. I have represented the steps and rail as well as I can in the figure.



AB is an iron rail supported by an iron baluster BC; BD is the same rail continued down the side of the steps, and supported at D by the iron baluster DE. The lightning, conducted (as I suppose) by the

the rail A B to B, and from thence by the baluster B C to C, struck off the corner of the stone step at C, without any discolouring of the step; the piece struck off might be three or four pounds weight. Part of the lightning, conducted farther from B to D along the iron rail was carried by the baluster D E to E, and a large piece was struck off from the corner E of the stone step; there was no discolouring of the step. The piece, which I took up in my hand, might be three or four pounds weight, and fitted the broken corner of the step exactly. This iron rail is within three feet of a leaden pipe, which comes down from the top of the house, and is not continued to the ground.

The lightning went up the east side of the street without any effect, till, at about the distance of 70 yards from the bottom house, it struck the flag pavement near the iron rails of the adjoining house, and broke off a piece of the flag stone, weighing, perhaps, two pounds; there was no discolouring here, but, as in the stone steps before mentioned, the appearance was as if the stone had been bidden by the blow of a sledge hammer. One continued leaden gutter runs over the eaves of these houses on the east side as well as on the west side.

The effects of the shock were very particular on some persons. A lady in the bottom house on the east side, who had left the room which looks over the river, to avoid the lightning, and sat near a window which looks directly up the street towards the north, fell from her chair; but her surprize was so great that she cannot say whether she was thrown down by the concussion of the air, or fell by the
fright.

fright. She says, she felt the lightning on her arm, and had a very odd sensation like what she supposes people feel by the electrical shock; she further says, her arm smelt very strong of sulphur for a considerable time, though she went out of the house immediately.

Another lady, who lives on the west side of the street, in the house the roof of which was bulged in, as has been mentioned, as she sat on the bed with a window open behind her, which looks to the west, was thrown off the bed on a child, who sat on a chair by the bedside. The sensation the shock gave her, was as it were of a blow cross her shoulders.

My house is on the east side of the street, next door but one to that where the steps were broken and the chimney thrown down. I was at home in the fore room on the ground floor. I felt a greater shock and concussion in the air than I had ever observed before from thunder. A gentleman, who was with me, says, what he felt was most like the sensation produced by the pressure of the water when a man leaps into it.

I am, Sir,

With great respect,

Your most humble servant,

Essex-street,
June 28, 1764.

Thomas Lawrence.

XLIII. *An Account of what appeared on
Opening the Body of an asthmatic Person:
By W. Watson, M. D. F. R. S.*

To the Royal Society.

Gentlemen,

Read July 12,
1764.

AS nothing tends more to illustrate the nature of uncommon diseases than the examination of morbid bodies after death, I take the liberty of communicating the following history.

Mr. W. aged twenty eight, consulted me at the latter end of November 1763. He had for about two months laboured under great difficulty of respiration, for which he had taken great variety of medicines without any relief. He had been largely bled, which, without helping his respiration, had greatly depressed and weakened him. The air of the country, where he had resided some time, made not the least alteration in his complaint, which daily grew worse.

When I saw him, his breathing was exceedingly difficult; the heat of his flesh moderate; his pulse too quick, beating about an hundred strokes in a minute, and withall low and thready, indicating great want of fluid in the arterial system. He coughed very frequently, and what he expectorated was viscid; but neither purulent nor bloody, and in no great quantity. He was without thirst. His nights were almost without sleep, on account of the difficulty

culty of respiration; he not having been able to lie down in bed for some time, more especially on his right side.

To relieve him, I ordered at intervals blisters to his legs and side, which, though they discharged liberally, did scarce help his respiration. I likewise during about ten days, that I saw him, directed solutions of the foetid gums, volatiles, *Conf. Damocratis cum Vino Antimon.* Vinegar of squills, volatile oily mixtures, and other antispasmodics and attenuants with plentiful dilution. The *Confect. Damocratis cum Vino Antimoniali* at first much relieved him, and he was enabled to lie down in his bed; but this relief was temporary.

He generally grew hot, and more uneasy towards evening; and one night, during my attending him, he was seized with a violent pain in his right side. For this he lost seven ounces of blood, which, though it somewhat sunk him, very much abated the violence of his pain.

Frequently likewise in the night, notwithstanding that the season was cold, he, on account of the difficulty of his respiration, ordered and insisted upon the windows of his chamber being opened, to gratify himself with yet cooler air than that of his chamber.

Two or three days before he died, his respiration was extremely laborious; he sweated profusely; and his strength growing less and less, he expired without the least convulsion.

Several days before his death, he took large doses of Vinegar of squills four or five times a day. This, though it did not in the least offend his stomach, did
not

not promote his expectoration, which still continued viscid.

From a careful consideration of this disease, I was of opinion, that it was confined to the lungs: that these, especially on the right side, adhered to the pleura: that their substance was occupied by tubercles, or something analogous thereto, which greatly disturbed their functions. The feverish heat and quick pulse I considered as symptomatic of, and occasioned by, his extremely laborious respiration.

As I was very desirous of seeing the state of his lungs after death, my request to satisfy myself was complied with; and this examination was sufficiently convincing, that the disease was of too severe a kind to admit of a cure.

Upon lifting up the sternum, the lungs were enormously distended with air, which no pressure could force back through the windpipe. This air was extravasate, had burst through the extremities of the *bronchia* and vesicular substance, and had insinuated itself throughout the whole substance of the lungs, in which it was detained by the membrane investing them. In a word, the whole substance of the lungs was in a state truly emphysematous. In several parts this air had formed large bladders, which, though no pressure upon the surface of the lungs could force back, a slight incision into them permitted to escape, and caused the whole lobe to collapse.

Besides this emphysematous affection of the whole substance of the lungs, the pulmonary vein was in all its parts distended into numberless *varices*, many

of which were of the size of the small, or Lucca olive, and were distended with grumous blood. Besides these, there was a larger cyst in the right lobe of the lungs, which was filled with deep-coloured ichor: this lobe adhered to the pleura in great part of its surface. The lungs in general were of a deep red colour, and here and there upon their surface beginning to sphacelate.

The figure of the human heart is that of a cone, divided through its axis; but in this case, the heart's figure was altered, and was more compressed than usual; and its ventricles distended with grumous blood.

Every other part of the body was in its natural state.

From this examination we find that in this instance respiration was greatly disturbed, to say nothing of the cyst in the right lobe, nor of the adhesion of that lobe to the pleura, from two manifest and potent causes; viz. the varicose state of the pulmonary vein, and the emphysema throughout the whole substance of the lungs.

The *varices* of the pulmonary vein not only retarded the blood in its passage to the left auricle of the heart; but, occupying a much larger space in the lungs than they naturally should, they left less room for the minute ramifications of the bronchia to extend themselves; and consequently a less quantity of air was taken in at every inspiration than was necessary for the ordinary purposes of life.

But the disorder of the lungs from the *varices* was made infinitely worse by the emphysema. For by the extravasate air possessing so large a portion of the lungs, and which the patient could by no means part
with

with in expiration, very little room was left for fresh air in inspiration; the lungs, from the emphysema, and from the diseased state of the pulmonary vein, filling almost the whole cavity of the thorax. This not only occasioned an enormous defect in the quantity of air in inspiration necessary to the purposes of life, but by the preternatural compression the motion of the blood was retarded in the lungs, more especially in their smaller vessels. This affected not only the serous extravasation in the cist before-mentioned, but occasioned those general obstructions in the blood vessels of the lungs, which brought on the sphacelated appearance; and finally, by the increase of the complaint, was the cause of death.

This extraordinary distension of the lungs accounted for the heart's being of a more compressed figure than is usually seen.

In the present instance an asthma was occasioned by two causes, either of which has hitherto been scarce considered as conducing thereto; the one an emphysema, and the other a varicose affection of the pulmonary vein. Had the causes of this disease been as perfectly known during the life of the patient, as since his death, the case would not have admitted of a cure; as there was no method of discharging the extravasate air from the lungs; neither could any medical process alter or amend the varicose state of the pulmonary vein.

Such a state of lungs, as that just now described, in an otherwise healthy young man, could not, I was persuaded, happen but from some very powerful cause; and, upon enquiry, I was informed, that about the beginning of October, not two months before his death, from something which had greatly offended his

his stomach, he was seized with violent and long continued vomitings. These, though at length they were quieted, left his chest very sore. From this time his cough became troublesome, as did remarkably his shortness of breath upon the least motion, attended with the several circumstances above described.

From considering the history of this disease, and comparing it with the appearance of the lungs after death, I cannot but be of opinion, that the violent efforts to vomit occasioned primarily both the emphysema, and the *varices* of the pulmonary vein. This opinion, I flatter myself, will not, to persons well versed in the animal œconomy, seem ill founded, when they reflect how forcibly the lungs are pressed in violent efforts to vomit, both by the muscles subservient to respiration and the abdominal muscles, as well as by the contents of the abdomen itself. And it is wonderful, when the texture of the lungs is considered, that accidents of this kind do not much oftener happen, not only in vehement reachings to vomit, but in violent coughs, pains of childbirth, lifting great weights, and other preternatural exertions of strength.

When once the extremities of the bronchia and the vesicular substance have given way, the mischiefs are easily foreseen. The air getting loose into the substance of the lungs cannot be parted with in expiration; it consequently is retained there, and the space it occupies prevents as much of the external air being received into the lungs as its own quantity. As, from their incessant motion, injuries to the lungs are not easily removed, when once a rupture is made, every fit of coughing or other violent exertion extravasates more air. Hence the rupture

ture still continuing, and probably increasing, more and more air becomes extravasate, until, as in the present case, the quantity becomes so great, as not only to impede the course of the blood through the lungs, but the internal pressure of the extravasate air prevents the ingress of a quantity of fresh air, sufficient to cool and attenuate the blood. In fact, a small part only of the lungs is employed; as the extravasate air, though still in an elastic state, answers by no means the purposes of fresh air in respiration; as the former, by its confinement in the lungs, is very soon divested of its vivifying spirit, that principle which is soon destroyed in animal bodies, and which some chemical physiologists have supposed to be an acid nitrous Gas, and is most essential to human life. Hence, in a very short time, the effects are too obvious to be mentioned; and death must soon follow, as happened to the person, who is the subject of this communication.

I am, with all possible regard,

Gentlemen,

July 6, 1764.

Your most obedient

Humble servant,

W. Watfon.

XLIV. *A Letter to the Marquis of Rockingham, with some Observations on the Effects of Lightning.*

To the most honourable the Marquis of Rockingham.

My Lord,

Read July 28, 1764. **I** Have taken the liberty of addressing the inclosed paper to your Lordship, as it contains a few observations on a subject not unknown to you, and may probably be thought of general use.

I have a further reason for addressing it to your Lordship, as it gives me an opportunity of expressing, in some small degree, the high sense of gratitude I bear your Lordship, for many and repeated favours and obligations conferred on,

My Lord,

Your Lordship's most obedient,

and most humble servant,

June 24, 1764,
Great Queen-street,
London.

Benjamin Wilson.

Considerations to prevent Lightning from doing Mischief to great Works, high Buildings, and large Magazines : By Mr. Wilson, F. R. S. and Member of the Royal Academy of Sciences at Upsal.

LONG experience, since the discovery by Dr. Franklin, has now established a truth amongst philosophers, that lightening, like the electric fluid, passes more freely through iron, copper, and other metals, than through dry wood, stone, or marble.

Instances of this truth are innumerable: and to convince us thereof, we need only trace the late violent effects of lightening on St. Bride's Church, and the houses in Essex-street, &c.

For, upon examining these buildings, it appears, that there are certain thick bars of iron, through which the lightening has past, without producing any visible effects: and on the contrary, in certain parts where the junctions of those bars with the stone, or wood, are made, there the lightening, rushing from the iron, has broke the stone to pieces, and shivered the wood.

From the like experience we also learn, that if the iron is too slender for conducting the lightening, it is either dashed into pieces, or exploded like gunpowder; just in the same manner as we are able, by the electric power, to break and dissipate in vapour a very slender wire. Bars of metal, of a proper thickness, and conveniently disposed, seem therefore necessary for the security of such buildings.

It is to be noted, that the mischiefs caused by lightening are not always owing to its direction from the clouds to the buildings or other eminences, and thence
to

to the earth; but sometimes, on the contrary, from the earth, buildings, and other eminences, to the clouds. For the principle upon which its direction depends, appears to arise from the restoration of a certain *equilibrium*, in a subtile and elastic fluid, previously disturbed by various causes.

Now, according to the laws of elastic fluids, the endeavour to restore the *equilibrium* of such a fluid, will be in that direction, where the *resistance* to it's passage happens to be the least. Upon this principle we therefore see a necessity, either to open a passage for it to go freely through, by placing certain bars of metal properly; or, to stop the passage of the fluid through such buildings entirely.

The last method would be dangerous to put in practice; because, if high buildings were so secured, the lightening would then attack the lower buildings, which are far more numerous, and probably would destroy a greater number of people, cattle, &c.

Whereas, if the first method is preferred, the high buildings will then tend to protect the lower ones more effectually; and may with propriety be considered, as so many pipes to carry off the lightening quietly; either from the earth to the clouds, or from the clouds to the earth.

And that several proper conductors are necessary to carry off the lightning more readily, than some of the *accidental* or *partial* conductors, in a large town, are capable of, appears from this; that we are able to collect small quantities of the electric fluid, with a slender apparatus in our hands only; whilst it is exposed in the street, garden, or other open place, during the hovering of such clouds as occasion violent lightening.

From repeated observations of this kind, there is reason to believe, the quantity of lightning at particular times is so very great, that it would be dangerous to invite it to any buildings, and that unnecessarily, in the most powerful manner we are able; by suffering the several conductors to end in a *point* at the top.

On which account, it is apprehended, *pointed bars*, or *rods of metal*, ought always to be avoided.

And as the lightening must visit us, some way or other, from necessity, to restore the *equilibrium*, there can be no reason to invite it at all: but, on the contrary, when it happens to attack our buildings, we ought only so to contrive our apparatus, as to be able to carry the lightening away again by such suitable conductors, properly fixed, as will very little, if at all, promote any encrease of it's quantity.

To attain which desirable end, in some degree at least, it is proposed, that the several buildings remain as they are at the top; that is, *without having any metal above them, either pointed or not*, by way of a conductor.

On the inside of the highest part of such building, and within a foot or two of the top, it may be proper to fix a rounded bar of metal, and to continue it down along the side of the wall to any kind of moisture in the ground.

But if the building happens to be mounted with an iron spindle, for supporting a vane, or other ornament, and it should not be convenient to have it taken away, then the bar of metal ought to communicate with that spindle.

And in regard to the diameter of such a metal bar, it will probably depend upon the height
 VOL. LIL. K k of

of the building: for it is apprehended the great church of St. Paul's, to compleat the partial conductors (which are the metallic cross, ball, gallery, dome, &c.) and secure it effectually, would require a bar of metal two inches diameter, if not more: and a building like the British Museum, one considerably less. But it appears there is no occasion for any at that repository, as it is already provided, though from *accident*, like many other buildings, with very effectual conductors. The copeings of the roof thereof, and the several spouts, which are continued from thence into the ground, being all of lead.

That conductors ought to be thicker than is generally imagined, seems to appear from a late instance taken notice of in St. Bride's church by Mr. Delaval and Dr. Watson, where an iron bar two inches and a half broad, and half an inch thick, or more, was bent and broke asunder by the violence of the lightning.

The Eddystone Lighthouse, which stands upon a rock surrounded by the sea, the work of Mr. Smeaton, was thought to be an object very likely to suffer by lightning; and the more so, as the top of it consisted of a copper ball two feet in diameter, with a chimney of the same metal, passing through it down to the second floor, but no further. Directions were therefore given to make a communication of metal from the lowest part of the copper chimney down to the sea; which was executed accordingly about the year 1760, or soon after the building was finished. Now if, instead of the copper ball, a pointed bar of metal had been put in its place, or above it, and communicated with the conducting matter below, there is no saying what might be the consequence of
so

so powerful an invitation, to an edifice thus particularly situated.

Read Nov. 8, 1764. **S**INCE the former part of this paper was communicated to the Royal Society, that is, on the 5th of August, 1764, I received the following account from captain Dibden, commander of a merchant ship, who says, that in the year 1759, he was taken by the French, and carried prisoner to Fort Royal in Martinico. That in removing him from thence some time after, and on foot to St. Pierre, which is about 20 miles, his conductor, or guard, stopped at a small chapel five miles from the last place, to shelter themselves from the heavy rain which fell during a violent thunder storm. That the chapel had no steeple or tower belonging to it, but stood upon an eminence with three or four poor low houses near it. That soon after they were thus sheltered, a violent flash of lightening struck two soldiers dead, who had been leaning against the wall of the chapel between two buttresses, and not far from the rest of the company ; they being all on the leeward side of the chapel.

That it made an opening in the wall about four feet high, and about three feet broad, and in that part only against which they rested.

That captain Dibden, along with other persons, entered at this hole immediately after, to see if any other damage had been done to the chapel. That they observed a square bar of iron near the hole, and upon the ground, about four feet long, and *one inch and a quarter thick*, making an angle with the wall, as they supposed, to support the upper part of an inclined tombstone, which was also thrown down and

broke to pieces. That this bar was joined in the middle to one end of another bar, about one foot long, and *one inch thick*, which laid horizontally, and, passing to the wall, had been there fastened with lead. That the lightening in rushing along the inclined bar, had wasted or reduced its thickness in some places very considerably: in so much, that it looked like a burnt pole which had been long used : and broke the bar into two pieces, about an inch above the joining of the lesser bar; the ends of which had a burnt flaky appearance. That the other parts of the bar were changed in colour to a grey, or whitish hue; resembling iron, after it has been exposed to a violent heat, and then suffered to cool.

That the horizontal bar had also undergone an extraordinary change by the lightening, but particularly at that end next the wall of the chapel, it being reduced from one inch in diameter, to the size of a slender wire, but *tapering towards the wall*.

That when the soldiers rested against the wall, their heads were about the same height with the shortest bar; and, from what he can recollect, were very near being opposite to that end thereof, which was inserted in the wall.

That the two soldiers were forced from the wall at the same instant by the lightening: so that their feet, which were one yard or more from it, were nearest to the wall, and their heads the farthest off. That their flesh appeared very black. That their cloaths were burnt and scorched in many parts: and their belts shriveled up, as if they had been exposed to a large fire. That captain Dibden, and other people, felt a disagreeable kind of an electric shock, at the same instant that the soldiers were killed.

Captain

Captain Dibden gave an account also, that he was lately at Virginia, 1763 : that the inhabitants of Norfolk had changed their opinions in respect to fixing of wires and small rods of iron on the tops of their houses ; from the frequent instances they have lately had of their being melted, or destroyed, by the violence of the lightening : and that now they adopted in their stead, rods of iron from half an inch thick, to three quarters of an inch thick, or more. That those rods ended in a point at the top, and extended from three feet above their houses down to the ground : and that many houses had one of these conducting irons at each end.

This account appears very material upon the present occasion, as it serves to confirm the conjectures that are now offered, in a manner so obvious as to require no particular explanation.

The captain added, that, though the pine trees are considerably higher than the oaks in the American woods, yet the oaks are the oftenest attacked by the lightening : and that he does not remember any oaks growing among the pine trees, when the latter have suffered by lightening : which must be owing to the greater *resistance* arising from the unctuous nature of the pine trees.

XLV. *Solis Defectus observatus in Collegio Romano a Patribus Societatis Jesu die prima Aprilis Anno MDCCLXIV. Tempore vero post mediam noctem.*

Read Nov. 8, ^{1764.} **I**N hoc observando Solari deliquio adhibuimus præcipue telescopium objectivo micrometro instructum, egregium opus Jacobi Shortii, a Prænestino principe iterum jam humanissime nobis commodatum, pro quo beneficio maximam illi, dum erimus, habebimus gratiam. Cum hoc instrumenti genere in Italia nulla sit hæctenus observata eclipsis, ab exteris vero nationibus, si quid hac in re præstiterunt, nihil ad nos pervenerit, quod & aliis pluribus contigisse arbitrari fas est, observationem hanc nostram, astronomis præsertim Italis, non injucundam fore censuimus.

Diffipata nebula raræ nubes, quæ superfuere, impedimenti quidem aliquid observationi attulerunt, ut plurimum tamen Sole satis claro usi sumus.

Ex observationibus decem diameter Solis deficientis, cui ex Pariensi Ephemeride respondent $1921''$, 4, deprehensa est intercipere micrometri particulas 2260: Summum autem inter omnes discrimen particulas duas non excessit.

Eclipsis initium animadversum est tubo palmorum decem $9^h 49' 8''$: Finis vero eodem tubo $12^h 52' 49''$.

Definivimus tum chordas obscurationis, tum distantias limborum Solis ac Lunæ secundum lineam
conjunct-

conjungentem utriusque centra, nulla digitorum habita ratione: maluimus enim tempus adnotare, quo aliquid accurate observatum videbatur; quam expectare, ut obscuratio certam mensuram attingeret, ne aut nubes, aut impedimenta, quæ loci angustię objecere plurima, sæpius observationem subriperent; nullus autem ad curiosos arcendos homines rem omnem perturbaturos opportunior locus occurrebat: eædem fuere causæ, cur hoc instrumentum adeo accurate aliqua non exhibuerit, ut in aliis pluribus experti sumus.

Chordæ limbi obscur.					Dist. limb. Solis, et Lunæ.				
h					h				
1	9	56	31	817	I.	10	6	59	1853
2	10	0	14	1016	II.		16	18	1636
3		5	0	1192	III.		33	3	1273
4	11	26	19	2095	IV.		56	38	825
5		31	17	2083	V.	11	9	49	656
6		34	11	2077	VI.		15	18	618
7		53	53	1949	VII.		21	45	618
8	12	11	53	1729	VIII.		39	32	798
9		21	32	1561	IX.		45	16	894
10		25	12	1484					
11		27	28	1432					
12		29	16	1388					
13		30	43	1349					
14		33	58	1257					

Ex observationibus VI, VII, VIII invenimus per interpolationem limborum distantias tempore observationum 4, 5, 6 ex ordine fuisse 639, 682, 716.

Ex

Ex datis eodem tempore Solis diametro, chorda partis obscuratae, & limborum distantia, in singulis observationibus Lunæ diameter in micrometri partibus ex ordine prodit 2114, 2110, 2114. Hinc Lunæ diameter circa horam 11 30' statui potest partium 2112,7, quibus respondent scrupula secunda 1796,1. In investigandis tamen centrorum distantibus, quas in sequenti tabula in scrupulis secundis damus, usi sumus constanti diametro 1795'', quia totum inter maximam & minimam discrimen Eclipsis tempore est circiter 3'', & ea, quam mox invenimus, media est aliquanto major.

DISTANTIÆ CENTRORUM.

Ex obs.	^h	'	''	'''		'	''	'''	
1	9	56	31	1723	6	11	34	11	540
2	10	0	14	1645	VIII.		39	32	615
3		5	0	1557	IX.		45	16	697
I.		6	59	1512	7		53	53	832
II.		16	18	1328	8	12	11	53	1134
III.		33	3	1019	9		21	32	1299
IV.		56	38	638	10		25	12	1363
V.	11	9	49	495	11		27	28	1403
VI.		15	18	462	12		29	16	1435
VII.		21	45	462	13		30	43	1461
4		26	19	472	14		33	58	1520
5		31	17	519					

Ex observationibus IV, VI, VII, VIII, mutuo comparatis minima centrorum distantia contigisse videtur 11^h 18' 45'', eaque fuisse 458'', adeoque maxima obscuratio digitorum 8 45'. Juniores aliqui ex nostris ex Solis imagine per tubum opticum in chartam transmissa obscuracionem maximam, nobili & erudita spectante

ſpectante multitudine, æſtimarunt digitorum fere $8\frac{2}{3}$, alius vero alibi eadem ratione obſervans digitorum $8\frac{1}{3}$, ac pluribus habitis circa illam obſervationibus vix $10'$ in tempore definiendo a noſtra ceterminatione defecit.

Lunæ diametrum ex eclipſi definitam cum diametro extra Solem conferre conati ſumus: verum die tertia Aprilis ægre admodum obſervari potuit propter nimiam Lunaris lucis tenuitatem, cui plurimum etiam officiebant reſiduum aliquod crepuſculi lumen, & in horizonſis viciniæ vapores. Eas proinde obſervationes, licet multum neque inter ſe, neque ab inventa diametro diſcreparent, rejecimus, ut certiores obtinere-
mus die quarta; ſed ea quoque die non levem experti ſumus difficultatem, quia Lunæ cornua in exiliſſimas tenuabantur lineas ab ejuſdem aſperitatibus umbra identidem interruptas: eas nihilominus, ut nobis ſeſe obtulerunt, hic ſubjicimus in micrometri particulis.

DIE QUARTA APRILIS.

Obſ.	^h	[']	Diam. Lunæ.
1	8	0	Vesp. 2096
2		7	2094
3		17	2093
4		23	2087
5		29	2088
6		35	2086

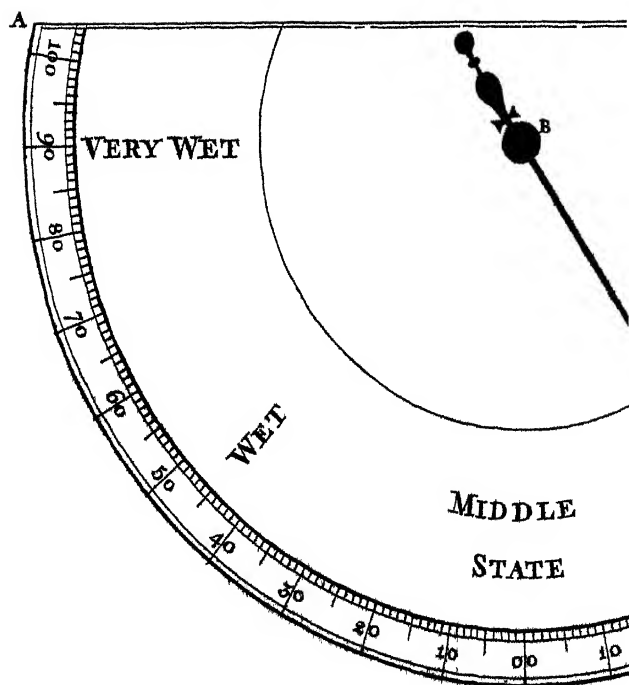
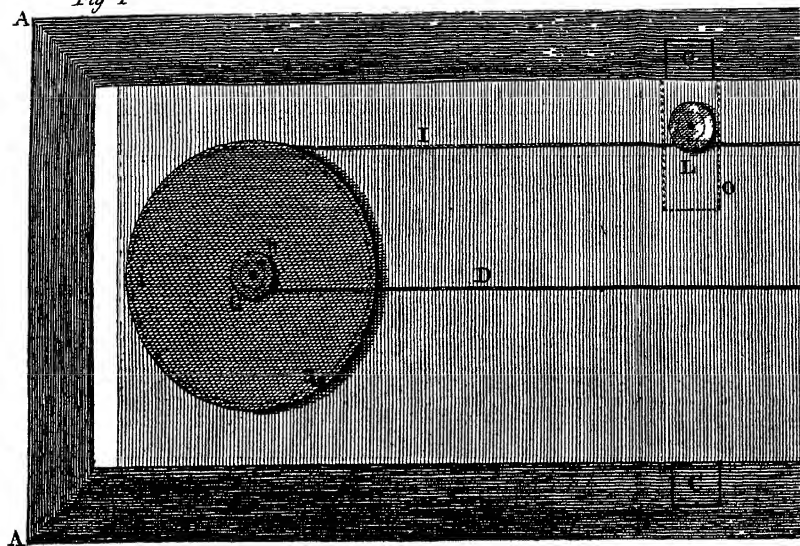
Converſis micrometri particulis in ſcrupula ſecunda, primo correximus diametri decrementum a refracti-
one ortum, nam diameter obſervata inclinabatur ad horizonſtem gradibus circiter 18, ut idem teleſcopium indicavit: hæc autem correctio in ultima obſervatione,

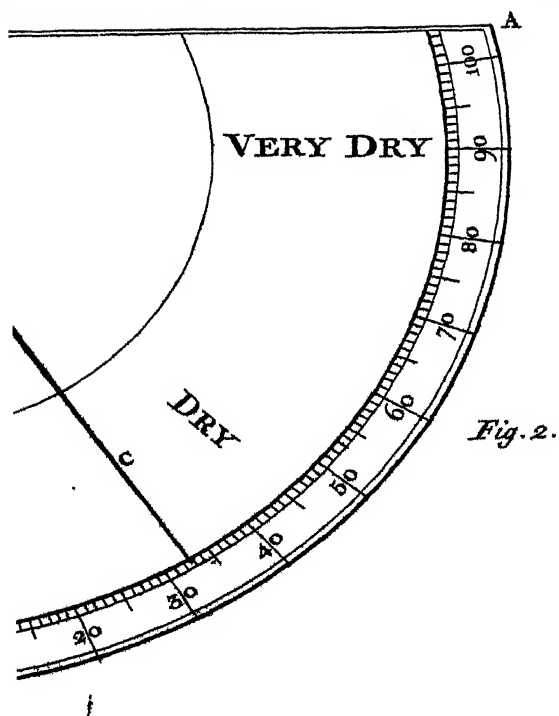
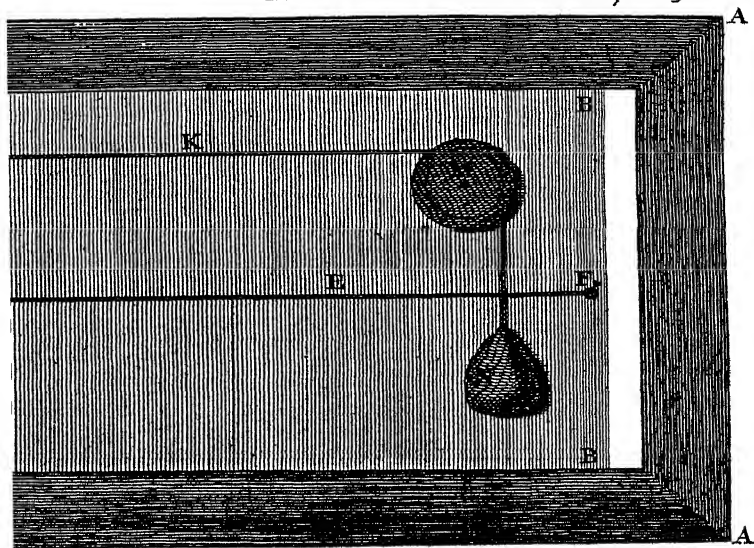
in qua maxima fuit, inventa est $1''$,2. Secundo, ex observationis hora determinavimus Lunæ horarium, ac declinationem, atque inde ejus altitudinem tum veram, tum apparentem. Tertio, ex iis investigavimus Lunæ diametrum horizontalem ope trianguli constituti a semidiametro terrestri, atque a rectis ductis a centro Lunæ ad spectatorem, & ad telluris centrum. Ex singulis observationibus hæc prodierunt.

Diam. horiz. Lunæ.		Diameter Lunæ horizon-
		talis die quarta Aprilis circa
Ex. obs.	1773, 7	horam octavam ex Parif.
2	1772, 8	Ephem. eadem est adamus-
3	1772, 9	sim, ac in ipsa eclipsi, in quo
4	1768, 4	sensibilis error esse non potest;
5	1770, 0	cum enim Luna circa Apo-
6	1769, 0	gæum versaretur, diametri
		variationes erant exiguæ, quas
Media	1771, 1	Astronomicæ Tabulæ exhibent satis accurate: tantum

augenda est ea quantitate, quam postulat apprens altitudo Lunæ $11^h 30'$, quæ fuit circiter grad. $52\frac{1}{2}$: quo facto additamento diametrum Lunæ obtinuimus $1793''$,2. Ab ea quam ex Eclipsi collegimus, discrimen est $3''$, quod ad unicum redigitur, si rejiciantur tres posteriores observationes, quas minus certas arbitramur, quia oculus prioribus defatigatus (summa enim opus erat contentione) fortasse factus est minus idoneus ad languidam Lunæ lucem percipiendam.

Fig 1





XLVI. *The Description of a New Hygrometer, invented by James Ferguson, F.R.S.*

Read Nov. 8, 1764. **I**N this machine (TAB. XVI. Fig. 1. and 2.) A A A A is a frame of waincot or mahogany, grooved in the innermost edges of the two longest sides, for holding a pannel B B B B of white deal-board, without pinching it. The pannel is about the thickness of a crown piece, and fifteen inches in length, cross-wise to the grain of the wood. The middle part projects outward from the upper and lower edges, at C and C, where it is fastened into the frame by two screws, to keep the middle part always in the same place, whilst the rest of the pannel expands by moist air toward both ends of the frame, and contracts toward the middle when the air is dry. F is a pin fixt into the pannel near one of its ends, and a round pin is fixt near the other end of the pannel, on which the large pulley H turns, and also the small pulley G which is fixed to H. One end of a small flexible cord D E is fastened to the pin F, and the other end goes round the pulley G, and is fixt into the bottom of its groove, as at *b*. One end of another small cord I K is fixed into the bottom of the groove of the large pulley H, as at *a*, from which it goes round the part *a* i H; and in its way thence to M it goes round a small pulley L, in which an axis is fixt; and turns in the piece O, which lies above the pulley, and is screwed to the upper side of the frame at C. This cord goes over the pulley M (which turns on a round pin fixt into the pannel) and has a flattish weight N hung to it.—The pullies G and L are of equal diameters in their grooves, which is only equal to a tenth part of the diameter of the large pulley H in its groove. The pulley M may be of any convenient size.

Now it is plain, that as much as the pannel expands between F and G, so much will the pulley G be removed farther from the pin F; and just so much will the cord D E turn the pulley G backward; and any point in the

groove of the pulley H ten times as much, because it is ten times the diameter of G in the groove: and this motion will cause the cord I K to turn the pulley L (and draw up the weight N) ten times as much as the pulley G is turned. So that, if the pannel expands a tenth part of an inch, by moist air, the pulley L will be turned quite round: and half round if the pannel expands but a 20th part of an inch: As the air grows dry, the pannel contracts, and the weight M descends and turns all the pulleys the contrary way.

The back of the plate A A (Fig. 2.) is screwed to the other side of the frame (Fig. 1.) so as the straight edge of the plate may be even with the uppermost side of the frame, and the center B (Fig. 2.) may be directly over the center of the pulley L (Fig. 1.) on whose axis the index BC (Fig. 2.) is fixed. And as the pulley L is turned by the cord I K, the index will be moved on the plate, and shew the degrees of moisture or dryness of the air.

If the expansion and contraction of the pannel be so great as to move the index beyond the limits of the degrees on the plate, this may be remedied by putting on a larger pulley at L. — But if not great enough, in very wet and very dry weather, to move the index through all the degrees on the plate, the pulley L must be made less in diameter accordingly.

N. B. In three or four years at most, a new pannel should be put into the frame: because, when the old one has been so long exposed to the air, it will almost cease to be affected thereby. And therefore, a large thick piece of deal should be kept in reserve for that purpose; and about the thickness of a card always planed off that side from which the new pannel is to be taken.

At G and M, there must be small knobs of some hard wood glued on the back of the pannel below the graduated plate, to make a proper thickness for holding the wires upright and fast on which the pulleys G and M do turn: for otherwise, the wires would soon loosen in the pannel.

XLVII. *Experiments and Observations on the Compressibility of Water and some other Fluids, by John Canton, M. A. and F. R. S.*

Read Nov. 8, 1764. **I**N a paper lately laid before the Royal Society *, I not only related the experiments by which I found water to be compressible, but also those by which I discovered how much a given weight would compress it when in a temperate degree of heat. By similar experiments made since, it appears that water has the remarkable property of being more compressible in winter than in summer; which is contrary to what I have observed both in spirit of wine and oil of olives: these fluids are (as one would expect water to be) more compressible when expanded by heat, and less so when contracted by cold. Water and spirit of wine I have several times examined, both by the air-pump and condenser, in opposite seasons of the year: and, when Fahrenheit's thermometer has been at 34 degrees, I have found the water to be compressed by the mean weight of the atmosphere 49 parts in a million of its whole bulk, and the spirit of wine 60 parts; but when the thermometer has been at 64 degrees, the same weight would compress the water no more than 44 parts in a million, and the spirit of wine no less than 71 of the same parts. In making these experiments, the glass ball containing the fluid to be compressed must be kept under water, that the heat of it may not be altered during the operation.

The compression by the weight of the atmosphere, and the specific gravity of each of the following fluids, (which are all that I have yet tried,) were found when the barometer was at $29\frac{1}{2}$ inches, and the thermometer at 50 degrees.

* See Philosophical Transactions, Vol. LII. p. 640.

	Millionth parts.	Specific gravity.
Compression of spirit of wine	66 —	846
Oil of olives	48 —	918
Rain-water -	46 —	1000
Sea-water - -	40 —	1028
Mercury - -	3 —	13595

These fluids are not only compressible, but also elastic: for if the weight by which they are naturally compressed be diminished, they expand; and if that by which they are compressed in the condenser be removed, they take up the same room as at first. That this does not arise from the elasticity of any air the fluids contain, is evident; because their expansion, by removing the weight of the atmosphere, is not greater than their compression by an equal additional weight: whereas air will expand twice as much by removing half the weight of the atmosphere, as it will be compressed by adding the whole weight of the atmosphere.

It may also be worth observing, that the compressions of these fluids, by the same weight, are not in the inverse ratio of their densities or specific gravities, as might be supposed. The compression of spirit of wine, for instance, being compared with that of rain-water, is *greater* than in this proportion, and the compression of sea-water is *less*.

The weight of $32\frac{1}{2}$ feet of sea-water is equal to the mean weight of the atmosphere: and, as far as trial has yet been made, every additional weight equal to that of the atmosphere, compresses a quantity of sea-water 40 millionth parts; now if this constantly holds, the sea, where it is two miles deep, is compressed by its own weight 69 feet 2 inches; and the water at the bottom is compressed 13 parts in 1000.

XLVIII. *Concise Rules for computing the Effects of Refraction and Parallax in varying the Apparent Distance of the Moon from the Sun or a Star; also an easy Rule of Approximation for computing the Distance of the Moon from a Star, the Longitudes and Latitudes of both being given, with Demonstrations of the same: By the Rev. Nevil Maskelyne, A. M. Fellow of Trinity College, in the University of Cambridge, and F. R. S.*

Read Nov. 15, 1764. **T**HE following rules, excepting one, are the same which I have already communicated to the Royal Society, but without demonstration, in a letter to the reverend Dr. Birch from St. Helena, containing the results of my observations of the distance of the Moon from the Sun and fixed stars, taken in my voyage thither, for finding the longitude of the ship from time to time; since printed in Part II. Vol. LII. of the Philosophical Transactions for 1762. The two rules for the correction of refraction and parallax I have also since communicated to the public in my British Mariner's Guide to the discovery of longitude from like observations of the Moon; and have added in the Preface a rule for computing a second but smaller correction of parrallax, necessary on account of a small imperfection

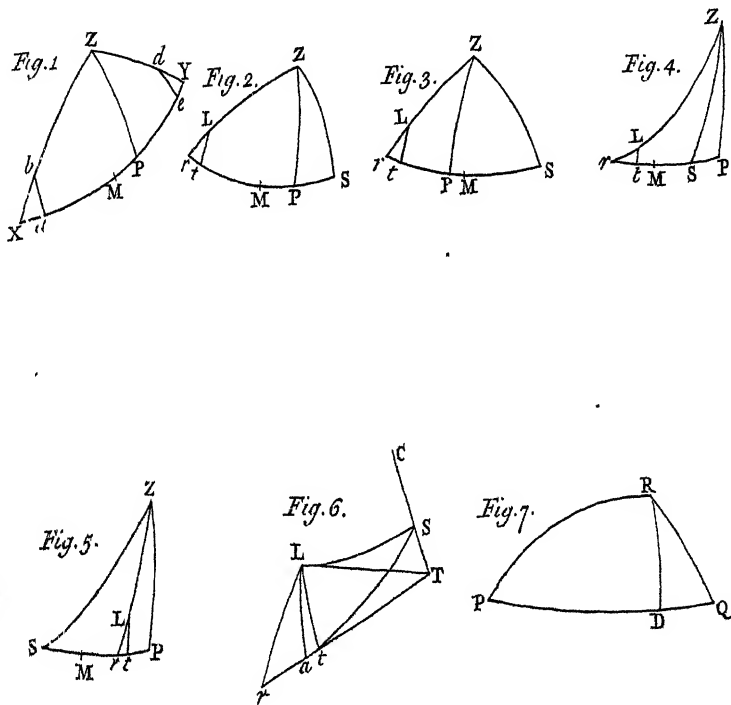
imperfection lying in the first rule derived from the fluxions of a spherical triangle. To the rules I have here subjoined their demonstrations.

With respect to the usefulness of these rules, I cannot but entertain hopes that they will appear more simple and easy than any yet proposed for the same purpose: the last rule, for computing the distance of the Moon from a star, though only an approximation, being so very exact, seems particularly adapted for the construction of a nautical Ephemeris, containing the distances of the Moon from the Sun and proper fixed stars ready calculated for the purpose of finding the longitude from observations of the Moon at sea; an assistance which, in an age abounding with so many able computers, mariners need not doubt they will be provided with, as soon as they manifest a proper disposition to make use of it.

A R U L E

To compute the contraction of the apparent distance of any two heavenly bodies by refraction; the zenith distances of both, and their distance from each other being given nearly.

Add together the tangents of half the sum, and half the difference of the zenith distances; their sum, abating 10 from the index, is the tangent of arc the first. To the tangent of arc the first, just found, add the co-tangent of half the distance of the stars; the sum, abating 10 from the index, is the tangent of arc the second. Then add together the tangent of double the first arc, the co-secant of double the
second



double the second arch, and the constant logarithm of 114'' or 2,0569: the sum abating 20 from the index, is the logarithm of the number of seconds required, by which the distance of the stars is contracted by refraction: which therefore added to the observed distance gives the true distance cleared from the effect of refraction.

Explication of the foundation of the preceding rule.

This rule is founded upon an hypothesis that the refraction in altitude is as the tangent of the zenith distance: and the refraction at the altitude of 45 degrees being 57'', according to Dr. Bradley's observations, therefore the refraction at any altitude, calling the radius unity is $= 57'' \times \text{tangent of the zenith distance}$. This rule is exact enough for the purpose of the calculation of the longitude from observations of the distance of the Moon from stars at sea as low down as the altitude of 10° , for there the error is only 10'' from the truth. But, if the altitude of the Moon or star be less than 10° , the rule may be still made to answer sufficiently, by only first correcting the observed zenith distances by subtracting from them three times the refraction corresponding to them taken out of any common table of refraction, and making the computation with the zenith distances thus corrected. This correction depends upon Dr. Bradley's rule for refraction, which he found to answer, in a manner exactly, from the zenith quite down to the horizon, namely that the refraction is $= 57'' \times \text{tangent of the apparent zenith distance}$

lessened by three times the corresponding refraction taken out of any common table.

Demonstration of the preceding rule.

Let ZXY TAB. XVII. Fig. 1. represent a spherical triangle formed by great circles joining the zenith Z and the stars X and Y . Refraction acting in the vertical circles ZX and ZY will carry the star X nearer the zenith by a quantity $Xb = 57'' \times$ tangent of ZX , and the stars Y towards Z by the quantity $Yd = 57'' \times$ tangent of ZY ; so that the apparent distance of the two stars will be bd instead of XY or less than XY , the true distance, by the Sun of the two little spaces Xa , Ye , terminated by the perpendiculars ba and de . The little space $Xa = Xb \times$ cosine of the angle ZXY (calling radius unity) $= 57'' \times$ tang. of $ZX \times$ cosine of angle ZXY , or, by spherics, $= 57'' \times$ tang. of XP . (ZP being an arch drawn from Z perpendicular to the arch XY). In like manner the little space $Ye = 57'' \times$ tang. of YP ; and therefore $Xa + Ye$ or the total effect of refraction $= 57'' \times \frac{\text{tang. } XP + \text{tang. } YP}{1 - tn}$. Let M be the middle of the arch XY , and put the tangent of XM or YM or $\frac{1}{2} XY = t$, and the tangent of MP , or the distance of the perpendicular ZP from M the middle of the arch $XY = n$. By trigonometry, tang. of XP or $XM + MP = \frac{t+n}{1-tn}$, and tang. of YP or $YM - MP = \frac{t-n}{1+tn}$; the sum of which, or tang. $XP +$ tang. $YP = \frac{t+n}{1-tn} + \frac{t-n}{1+tn} = \frac{2t+2tn^2}{1-t^2n^2} = \frac{2tn}{1-t^2n^2} \times \frac{1+n^2}{2n} \times 2$. Now

$2tn$

$\frac{2tn}{1-t^2n^2}$ is the tangent of double the angle whose tangent is tn , and tn or the product of the tangents of $\frac{1}{2}XY$ and MP , by spherics, is equal to the product of the tangents of half the sum and half the difference of the zenith distances ZX and ZY ; whence $\frac{2tn}{1-t^2n^2}$ is equal to the tangent of double arch the first found by the rule. Also arch the second found by the rule being by spherics $= MP$, whose tangent is represented by n , and $\frac{1+n^2}{2n}$ being by trigonometry equal to the cosecant of double the arch whose tangent is n , therefore $\frac{1+n^2}{2n} = \text{cosecant of } 2MP \text{ or double arch the second.}$ Whence the rule is manifest; namely, that $Xa + Ye$, the total effect of refraction in contracting the apparent distance of the two stars $= 57'' \times \tan. XP + \tan. YP = \text{double } 57'' \text{ or } 114'' \times \text{tang. of double the first arch} \times \text{cosecant of double the second arch. Q.E.D.}$

R E M A R K.

When the perpendicular arch ZP , let fall from the Zenith on the arch XY , falls without the triangle ZXY , the effect of refraction in diminishing the apparent distance of the stars X, Y is the difference of Xa and Ye : but the rule being general, gives always the sum or difference, which-ever it be, which is a great advantage, and removes all grounds of ambiguity in the correction of refraction; as the the total effect thereof always diminishes the distance of two stars from each other, however they are posited.

A R U L E

To compute the contraction or augmentation of the apparent distance of the Moon from a star, on account of the Moon's parallax ; the zenith distances of the Moon and star, and their distance from each other being given nearly.

Add together the tangents of half the sum, and half the difference of the zenith distances of the Moon and star, and the cotangent of half the distance of the Moon from the star ; the sum, abating 20 from the index, is the tangent of an arch, which call A. Then, if the zenith distance of the Moon is greater than that of the star, take the Sum of the arch A, just found, and half the distance of the Moon from the star ; but, if the zenith distance of the Moon be less than that of the star, take the difference of the said arch A and half the distance of the Moon from the star ; and the sum, or difference call B. To the tangent of B, thus found, add the cosine of the Moon's zenith distance, and the logarithm of the Moon's horizontal parallax, expressed in minutes and decimals ; the sum, abating 20 from the index, is the logarithm of the effect of parallax, tending always to augment the apparent distance of the Moon from the star ; except the zenith distance of the Moon be less than that of the star, and, at the same time, the arch A be greater than half the distance of the Moon from the star, in which case the effect of parallax diminishes the apparent distance of the Moon from the star.

DEMONSTRATION.

In the spherical triangle ZLS , see Fig. 2, 3, 4, and 5th, Z represents the zenith, L the Moon, and S the star; the effect of parallax depressing the Moon from L to r , r is the apparent place of the Moon, and rS the apparent distance of the Moon from the star; let fall the perpendicular Lt upon rS , produced if necessary, and rt will be the difference of LS and rS , or the effect of parallax. Draw the arch ZP perpendicular to rS , and let M be the middle of rS . The Moon's parallax in altitude, being to her horizontal parallax, as the sine of her apparent zenith distance, to the radius, $Lr = \text{Moon's horizontal parallax} \times \text{fine of } Zr$; and rt the effect of parallax upon the apparent distance of the Moon from the star will be $= Lr \times \text{cof. } ZrS = \text{horizontal parallax} \times \text{fin. } Zr \times \text{cof. } ZrS$ (or, because $\tan. rP : \text{cof. } ZrP :: \tan Zr : \text{rad} :: \text{fin } Zr : \cos Zr$; and therefore $\text{fin. } Zr \times \cos ZrS = \text{cof. } Zr \times \tan rP$) $= \text{horizontal parallax} \times \text{cof. } Zr \times \tan. rP$ agreeably to the rule. For it is evident by spherics that the arch A , found by the rule, is the same with MP the distance of the perpendicular from the middle of the arch rS : and it is evident, by the inspection of the figures, that the arch B or rP is equal to the sum of rM and MP , if the zenith distance of the Moon be greater than that of the star, as in Fig. 2d and 4th; but is the difference of rM and MP , if the zenith distance of the Moon be less than that of the star, as in Fig. 3d and 5th. Lastly, it may appear from the
consideration

consideration of the figures, that, as the effect of parallax depresses the Moon directly towards the horizon, so it will always encrease her apparent distance from a star, except in the case represented by Fig. 5th; that is to say, unless the zenith distance of the Moon be less than that of the star, and, at the same time, the arch MP be greater than $\angle M$ or half the distance of the Moon from the star. Q. E. D.

Remarks on the use of the two foregoing rules.

It has been remarked, after the rule for refraction above, that if the altitudes of the Moon or star are under 10 degrees, the zenith distances must be first lessened by 3 times the refractions corresponding to their respective altitudes before the effect of refraction be computed.

But in order to compute the effect of parallax from the second rule, the observed distance of the Moon from the star must be first corrected by adding the effect of refraction to it; found by rule the first; as must the observed altitudes of the Moon and star be also corrected by taking from them their respective refraction in altitude, and the corrected arches thus found must be made use of in computing the parallax. Only, if the altitudes of the Moon and star are both 10 degrees or more, part of the calculation of rule the second may be saved, and arch the second, found by rule the first, taken for arch A in the second rule without any sensible error. In this case, it will be most convenient to observe the following order of computation instead of that before prescribed

prescribed to be used when the altitudes are under 10 degrees.

1st. Making use of the apparent altitudes of the Moon and star uncorrected, compute arches the first and second by the directions contained in the rule of refraction.

2dly. Taking arch the second for arch A in the rule of parallax, compute the effect of parallax according to rule the second.

3dly. With arches the first and second compute the effect of refraction by rule the first.

4thly, and lastly. Applying the two corrections of parallax and refraction duly, according to the rules, to the observed distance of the Moon from the star, you will have the true and correct distance of the Moon from the star, cleared both of refraction and parallax.

A R U L E

For computing a second, but smaller correction than the first, necessary to be applied to the observations of the distance of the Moon from a star on account of parallax.

Call the principal effect of parallax, found by the preceding rule, the parallax in distance; and find the parallax answering to the Moon's altitude. Then to the constant logarithm 0.941 add the logarithm of the sum of the parallax in altitude and the parallax in distance, the logarithm of the difference of the same parallaxes, and the cotangent of the observed distance of the Moon from the star (corrected for refraction, and the principal effect of parallax), the sum, abating 13 from the index, is the
logarithm

logarithm of the number of seconds required, being the second correction of parallax; and is always to be added to the distance of the Moon from the star, first corrected for refraction, and the principal effect of parallax found above, in order to obtain the true distance; unless the distance exceeds 90 degrees, in which case it is to be subtracted.

D E M O N S T R A T I O N.

Let L Fig. 6. represent the Moon's true place in the sphere, and r her apparent place as depressed by parallax, S the place of the star, and Lt a perpendicular let fall from the true place of the Moon L upon the great circle rS joining the star S and the apparent place of the Moon r ; (all as in the four figures belonging to the preceding rule). Let La be the arch of a parallel circle described from the star S as a pole through the true place of the Moon L . Sa terminated by the parallel circle La , and not St terminated by the perpendicular Lt , as was supposed in the former demonstration, is equal to SL or the true distance of the Moon from the star, which was therefore computed too small from the former rule by the little space at . Let LT and aT be the equal tangents of the equal arches LS and aS in L and a , meeting in the radius CS , drawn from the centre of the sphere C and produced, in T . The space Lat , on account of its smallness, may be looked upon as lying all in one plane namely LaT , and La as the small arch of a circle described from the point T as a centre with the line LT as a radius,

thro'

through L and a, Lt as the sine, and at as the versine of the arch La; and consequently at equal to the square of Lt divided by 2 LT. But, the triangle Lrt being right-angled in t, the square of Lt is equal to the difference of the squares of Lr and rt, and consequently to the product of their

sum and difference; that is to say, $at = \frac{\overline{Lr+rt} \times \overline{Lr-rt}}{2 LT}$

or (because the tangent TL is equal to the square of the radius CS divided by the cotangent of LS)

$= \overline{Lr+rt} \times \overline{Lr-rt} \frac{\times \text{cotangent of } LS}{2 \times \text{square of } CS}$. Now sup-

pose the spaces Lr, rt to be expressed in minutes, which will be most convenient in practice, then the radius of the sphere CS must be taken equal to $3437\frac{3}{4}$, for so many minutes are contained in an arch of a circle equal to its radius: and at will be =

$\frac{\overline{Lr+rt} + \overline{Lr-rt} \times \text{cotan. of } LS}{2 \times 3437\frac{1}{4} \times 3437\frac{3}{4}}$. But, the cotan-

gents of similar arches of circles of different radii being directly as the radii, therefore the cotangent of LS to the radius CS or $3437\frac{3}{4}$, is to the cotangent of the same arch to 10000000000, which is the radius to which the logarithmic tables are adapted, its logarithm being 10; as $3437\frac{3}{4}$ to 10000000000.

Therefore the cotangent of LS = tabular cotangent of LS $\times 3437\frac{3}{4}$, which, being substituted in the value

of at above, gives at, expressed in minutes = $\frac{\overline{Lr+rt} + \overline{Lr-rt} \times \text{tabular cotangent } LS}{6875500000000}$; or, multi-

plying by 60, the value of at will come out in seconds

$$\text{conds} = \frac{\overline{Lr + rt} \times \overline{Lr - rt} \times \text{tabular cotangent of } LS}{1146000000000}$$

The logarithm of the denominator 1146000000000 is 12,059, instead of subtracting which, when the operation is performed by logarithms, add 0,941 (its compliment to 13) to the value of the numerator found in logarithms, and subtract 13 from the index: the remainder will be the value of *at* in seconds. Q. E. D.

A concise rule to find the distance of the Moon from a zodiacal star, very nearly; the difference of the longitudes of the Moon and star, and the latitudes of both being given.

To the cosine of the difference of the longitudes add the cosine of the difference of the latitudes, if both of the same denomination, or sum; if of contrary denominations, the sum of the two logarithms, abating 10 from the index, is the cosine of the approximate distance. This gives the true distance of the Moon from the Sun, being then nothing more than the common rule for finding the hypotenuse of a right-angled spherical triangle from the two sides given. But in the case of a zodiacal star apply the following correction to the approximate distance thus found.

To the constant logarithm 5,3144 add the sine of the Moon's latitude, the sine of the star's latitude, the versè-sine of the difference of longitude, and the cosecant of the approximate distance; the sum of these 5 logarithms, abating 40 from the index, is the logarithm of a number of seconds, which subtracted

tracted from the approximate distance, found before, if the latitudes of the moon and star are of the same denomination, or added thereto, if they are of different denominations, gives the true distance of the Moon from the star.

N. B. This rule, though only an approximation, is so very exact, that even, if the latitude of the Moon was 5° , and that of the star 15° , the error would be only $10''$; and if the latitude of the Moon be 5° , and that of the star 10° , the error is only $4'' \frac{1}{2}$; and if the latitudes be less, will be less in proportion as the squares of the sines of the latitudes decrease.

DEMONSTRATION.

Let P [Fig. 7.] represent one of the poles of the ecliptic and Q, R the places of the Moon and star. From R let the arch of a great circle R D be drawn perpendicular to P Q. By spherics, the tangent of P D = tangent of P R \times cosine of the angle R P D. And, by trigonometry, cosine of Q D or (Q P - P D) = cos. Q P \times cos. P D + sin. Q P \times sin. P D = cos. Q P \times cos. P D + sin. Q P \times cos. P D \times tan. P D = cos. P D \times cos. Q P + sin. Q P \times tan. P D = cos. P D \times cos. Q P + sin. Q P \times tan. P R \times cos. P \times cos. Q D : cos. P D :: cos. Q P + sin. Q P \times tan. P R \times cos. P : 1. But, by spherics, cos. Q D : cos. P D :: cos. R Q : cos. P R \therefore cos. R Q : cos. P R :: cos. Q P + sin. Q P \times tan. P R \times cos. P : 1. Whence cos. R Q = cos. P R \times cos. Q P + sin. Q P \times sin. P R \times cos. P : Now, by trigonometry, cos. (Q P - P R) = cos.

$= \text{cof. } QP \times \text{cof. } PR + \text{fin. } QP \times \text{fin. } PR;$
 whence $\text{fin. } QP \times \text{fin. } PR = \text{cof. } (QP - PR) -$
 $\text{cof. } QP \times \text{cof. } PR;$ which being substituted above,
 gives $\text{cof. } RQ = \text{cof. } (QP - PR) \times \text{cof. } P - \text{cof. } PR \times \text{cof. } QP \times \text{cof. } P + \text{cof. } PR \times \text{cof. } QP =$
 $\text{cof. } (QP - PR) \times \text{cof. } P + \text{verse-fine } P \times \text{cof. } PR \times \text{cof. } QP.$ Now put $\text{cof. } (QP - PR) \times$
 $\text{cof. } P = \text{cof. } G,$ or the approximate distance, then
 $\text{cof. } RQ - \text{cof. } G,$ or (because the difference of
 RQ and G is but small) $\overline{G - RQ} \times \text{fin. } \left(\frac{G + RQ}{2} \right)$
 $= \text{verse-fine } P \times \text{cof. } PR \times \text{cof. } QP$ nearly.
 Whence $RQ = G - \frac{\text{verse-fine } P \times \text{cof. } PR \times \text{cof. } QP}{\text{fin. } G + RQ}$
 nearly $= G - \frac{\text{v. f. } P \times \text{cof. } PR \times \text{cof. } QP}{\text{fin. } G}$ nearly.
 Q. E. D.

Note, the error of this formula arises from
 taking $G = \frac{G + RQ}{2}$ by which means it will always
 give RQ too great, nearly by the following quan-
 tity, $\frac{1}{2} S q \text{ cof. } PR \times S q \text{ cof. } QP \times \text{cot. } G \times S q$
 $\tan. \frac{1}{2} G.$ This comes to a maximum when G is
 60° , and is then $= \frac{1}{8} \sqrt{\frac{1}{3}} \times S q \text{ cof. } RP \times S q \text{ cof. } PQ.$ If the latitudes of the Moon and star are both
 5° it is $= 1''.$ If the Moon's latitude be 5° , and that
 of the star 10° , it is $= 4'' \frac{1}{2};$ and if the latitude
 of the star be 15° it is $= 10''$

XLIX. *Extract of a Letter from Mr. John Winthrop, Professor of Mathematicks in Cambridge, New England, to James Short, A. M. F. R. S.*

S I R,

Dated June 6, 1764.

Read Nov. 15, 1764. **I** AM greatly obliged to you for your candid and judicious remarks on my observation of Venus on the Sun, which I received from my much-esteemed friend Dr. Franklin. I wrote to the Dr. pretty largely on the subject, which I desired him to communicate to you : but when I had the pleasure of a visit from him last summer, he could not recollect whether he had done it or not. I therefore beg leave now to trouble you with the substance of it. Your remarks turned on two points, the longitude of the place of observation, and the equation of time when found by equal altitudes. As to the first, I was so diffident of the observation on the Moon, that I chose to keep to the longitude of St. John's, as set down by Sir Jonas Moore, who makes it $52^{\circ} 50'$ West of Greenwich. Though I did not think it needful to mention this doubt in the pamphlet, which was published soon after I got home, to gratify the curiosity of my countrymen, yet I expressed it fully in a written account of the observation, drawn up in a different form, and sent to the late Dr. Bradley, but which I believe never reached his hands.

As-

As to the equation of the time of noon, depending on the change of the Sun's declination, I did not make it in that pamphlet. I had all along intended to calculate it when I should settle the result of the observation, though I knew it must be very small, as the Sun did not alter his declination, then, above 1' in 4 hours. But when I came to observe the contacts, and found that I could not be sure of them within 3'' or 4'', whereas Dr. Halley's papers led me to expect that I might observe them to a single second, I thought it hardly worth while to calculate this equation, as the precise moments of the contacts could not be fixed by it.—But I have since done it, and find it to be 4'' to be subtracted from the middle time. An account of the observation, thus corrected, I sent last summer to Professor Bliss at Greenwich, concluding, from his station, that the observations from different parts of the world would be collected and compared by him. But having since understood that that affair is in your hands, though I have not yet had the pleasure of seeing your paper upon it, and having had no return from Mr. Bliss, I ask leave to transmit a copy of it to you; and if you will be pleased to give it a place in your Transactions with a remark at the end (if you think it proper) showing what the Sun's parallax comes out from the observation as it now stands (which I suppose will not differ much from the other determination.) I shall take it as a favour. This will do honour to the government who employed me. Perhaps, too, posterity may be glad to see, and may make use of, the only observation of this rare phenomenon that was made in America.

I am, &c.

John Winthrop.

L. Obser-

L. *Observation of the Transit of Venus, June 6, 1761, at St. John's, Newfoundland: By John Winthrop, Professor of Mathematicks and Philosophy at Cambridge, New England.*

Read Nov. 15, 1764. **T**HE transit of Venus over the Sun, being a very curious and important phenomenon, engaged the attention of America as well as Europe. His excellency Francis Bernard, Esq; governor of the Massachusetts-Bay, a gentleman who seizes every opportunity of advancing the sciences, was desirous to have an observation of it in this quarter of the world; and as Newfoundland was the only British plantation where one could be had, proposed to the General Assembly at Boston to make provision for that purpose, which they readily agreed to do. In consequence whereof, I embarked on board a vessel in the service of the government, taking with me for assistants two young gentlemen my pupils; and such astronomical instruments out of the college apparatus as were necessary. These were, an excellent clock, Hadley's octant with nonius divisions; a refracting telescope with wires at half right angles, for taking differences of right ascension and declination; and a nice reflecting telescope, adjusted by cross levels, and having vertical and horizontal wires, for taking correspondent altitudes; or differences of altitudes and azimuths.

We arrived at St. John's Newfoundland the 22d of May, where we met with a very kind reception, and all the assistance we could desire. As this town is bounded with high mountains toward the sun-rising, so that no house in it would answer our end, we encamped on an eminence at some distance, from whence we could see the Sun presently after his rising.

Hither we conveyed our instruments, and secured the clock to a pillar set in the ground under a tent. Near this tent, and within call of the clock, we fixed two other pillars firmly in the ground; one, to mount the refracting telescope on; the other, which was above 8 feet high, for a style or gnomon, having at top a plate of lead with a little hole for transmitting the Sun's rays; and we laid an horizontal platform to receive those rays. The platform we kept covered, to defend it from the Sun and weather: and examined its position every time we made use of it, by a very long level. On this we carefully drew a meridian line, by correspondent altitudes of the Sun, taken both by the reflector and by the Sun's image on the platform. These operations we repeated every fair day, and several times in a day. It would be tedious, as well as needless, to give a detail of them: 'tis sufficient that we adjusted the clock with as much exactness as we could have done at home.

Thus prepared, we waited for the critical hour, which proved favourable to our wishes. The morning was serene and calm. The Sun rose behind a cloud that lay along the horizon, but soon got above it; and at 4^h 18' we had the pleasure of seeing *Venus on the Sun*; though dimly indeed at first. But
I
the

the planet presently became distinct, and her limb well defined. Upon this, I applied myself to observe the passage of the Sun's and Venus's preceding limbs, by the vertical, and of their lower limbs by the horizontal, wires in the reflector, and made the following observations ; one of my assistants counting the clock, and the other writing down the observations as I made them ; which, having made the proper correction of the time for the change of the Sun's declination, stand as follows.

True time.			diff. long. ☉ & ♀		Lat, South.	
	^h	^m ^s				
At 4	21	20	Sun at the vertical ———		10	47
	31		Venus at the same ———		9	2
23	6		Venus at the horizontal —		9	4
24	23		Sun at the same. ———		10	50
27	29		Venus at the horizontal —		9	25
28	47		Sun at the same ———		10	52
35	15		Sun at the vertical ———		10	55
	21		Venus at the same ———		9	56
37	49		Venus at the horizontal —		10	8
39	9		Sun at the same ———		11	00

As Venus began now to draw near the Sun's limb, I prepared to observe her egress. The interior contact did not appear so perfectly instantaneous, as Dr. Halley's papers led me to expect. I was not certain of it till 4^h 47' 21'', though I doubted of it at 17''. The exterior contact I judged to be at 5^h 5' 49'', doubtful also 3 or 4'' ; and so the passage of Venus's diameter, 18' 28''.

The above observations gave me several altitudes and azimuths of Venus, from whence I deduced her

right ascensions and declinations; and from them, her longitudes and latitudes. The result of the whole, or the planet's difference in longitude from the Sun's centre and her latitude, is set down above, against each observation of Venus.

From hence I concluded that at the central emersion, which I put at $4^h 56' 38''$, the difference of longitude was $11' 19$, and the latitude $11' 6''$. Also, that the conjunction in longitude happened at $2^h 4' 36''$, the planet's latitude then being $9' 28''$.

In these calculations, I supposed the semidiameter of the Sun to be $15' 50''$, and of Venus $29''$.

By several observations, I found the latitude of the place $47^\circ 32' N$; which falls within the latitudes laid down in several books and maps, which make it from $47^\circ 25'$ to $48^\circ 0'$. I could make no use of Jupiter's satellites in finding the longitude, as they were not risen high enough to be observed above an hour before day-light came on. There were but two of their eclipses that could have been visible there while I was on the island; and though I watched for both of them, I was disappointed of both by unfavourable weather. Neither was I fortunate enough to get so much as one occultation of a fixed star by the Moon, though I spared no pains for it. The only observation I could get for this purpose was of the right ascension of the Moon, which I endeavoured to find, by comparing with that of a fixed star. But whether any mistake was committed in counting the clock, or in writing down the observations, or whether the position of the telescope was disturbed by any accident in the interval between the Moon's and star's passing, I am not able now to say. However, as I am sensible
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that observation is not to be depended on, I think it needless to insert it here. The longitude of St. John's is variously set down by different authors, though none I have met with mention the observations by which it was determined. According to Sir Jonas Moore, it is $52^{\text{h}} 50'$ West from Greenwich; and as his authority may be as good as any, I keep to this longitude till it can be ascertained by farther observations. I have taken measures to procure such; and if they succeed, shall be ready to communicate the result.

I viewed the Sun with great attention in the reflector both on the 5th and 6th of June, in hopes to find a satellite of Venus; but in vain. There were several spots then on the Sun; but none that I saw could be a satellite.

The variation of the needle there I found 19° W.

J. Winthrop.

R E M A R K.

Mr. Short has computed the parallaxes at the egress for this observation at St. John's, and by comparing this observation with that at the Cape of Good Hope (on the above longitude and latitude of St. John's as set down by Mr. Winthrop) he finds the parallax of the Sun, resulting therefrom, $= 8'',25$.

LI. *An Account of the Effects of Lightning on three Ships in the East-Indies: By Mr. Robert Veicht. Communicated by William Lewis, M. D. and F. R. S.*

Read Nov. 22, 1764. **A**UGUST the 1st 1750. Lat. $1^{\circ} 56' N$. Malacca bearing about N. E. All this day there was a fresh breeze, S. to S. S. E. the weather being hot and sultry. The evening was fair and clear; and when day-light was gone, there was not a cloud in the sky; and the water was so clear of vapour or mist, that the stars could be seen to rise out of the horizon.

This serenity continued till about 2 A. M. when a black cloud appeared above the horizon in the W. and W. N. W. and continued to rise very fast; and the flashes of lightening, which proceeded from it, succeeded each other very fast. In $\frac{1}{4}$ of an hour, it covered almost half the hemisphere, and as it approached, the wind from the S. E. began to fail, and died quite away at last.

By the time the clouds had covered half the hemisphere, the wind proceeded from it in great violence, and the flashes of lightening were very frequent, and we judged of their nearness to the ship by the interval betwixt the flash and report, according as this interval was longer or shorter. The whole heavens were now covered with this cloud, and the flashes of lightening happened at times on different sides of the ship, which had all the sails
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furled

furled before it came upon her. It must be remarked, that the wind, which reached the ship before the thunder, brought with it a violent and heavy rain, which sufficiently soaked the ship and every thing about her. The ship was all this time, which was in about half an hour after its first appearance above the western horizon, in the midst of repeated flashes of lightening, which were just upon the ship by her trembling and shaking on every explosion, and the flash and clap coming in the same instant, and the officers and people were apprehensive of damage to the mast.

2 $\frac{1}{2}$ A. M. At this time a clap burst, as was judged by the report, about mid-way betwixt the head of the mast and the body of the ship, or it might be higher, and in descending might cause that appearance, and just over it. This made the ship tremble and shake as if she was going to burst into pieces, and great pieces and splinters of the mast were fallen upon different places of the ship; but it was so very dark, we could not see from which of the masts they were forced.

Immediately after this first came a second, which burst just above, and on the quarter deck of the ship, which by the report was as great, and being close upon the deck was more terrifying than the former.

Here I must take notice, that the wind brought very heavy rain before the thunder came near the ship; and in proportion as the thunder approached the ship, the wind, which came in violent gusts at first, decreased gradually, and the rain was less heavy; and when the thunder surrounded the ship, and broke upon her, it was almost calm. And we could

could not only judge of the nearness of the thunder by the tremor and shaking of the ship, and the report instantaneously following the flash ; but we could also hear several of the flashes fall into the water close upon the ship.

We reckoned, that the first clap, which burst at the main-mast, was what damaged the mast ; the second having burst betwixt the main-mast and mizen-mast.

At this time we came to an anchor, and continued till day-light, that we might examine into the damage we had received ; for, as pieces of the mast were carried to all places of the ship, we imagined, that all the masts had been hurt.

At day-light we found, that the fore-mast and mizen-mast had escaped, and the main-mast had suffered as follows :

All the main-top-gallant-mast (which is the uppermost piece of the mast) from the rigging at the top of it, to the cap at the head of the main-top-mast, was entirely carried away, part falling over-board, and part into the ship in different places. The main-top-mast had great pieces carried from it, from the hunes down to the cap, at the head of the main-mast, so that it could but just stand, being hardly strong enough to bear its own weight, and that of its rigging. The main-mast being composed of three pieces, towards the top of it, those of the sides, being of oak, called the cheeks, were not hurt ; but the middlemost part, being of fir, was shivered in several places, and pieces were carried out of it 6 or 7 inches in diameter, and from 10 to 12 feet long, and this in a circular descending manner from the parrel of
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the main-yard down to the upper deck of the ship, the pieces being taken out crooked, or circular, or strait, according as the grain of the wood ran. It must be remarked, that these claps were not one single explosion, but successive explosions, about the dimensions, as near as we could guess, of small shells, and continued some time cracking after each other; and as the lightning is observed to run not in strait line, but zig zag, so these different explosions might be differently placed in the air; that when they came to take fire and burst, they might take the pieces out of the different sides of the mast as above related.

In great ships the masts are composed of three parts, erected upon one another, the lowermost part is called by its proper name, the middlemost part is called the top-mast, and the uppermost part the top-gallant-mast. The mast, which was here damaged, was the main-mast, or principal mast of the ship, and which stands near the middle; and sometimes the name of main-mast is applied to all the three pieces as they stand erected, and sometimes to the lower piece, or part of the mast only: and when they are distinguished severally, they are called the main-mast, main-top-mast, and main-top-gallant-mast.

Each of these parts of the mast are divided as to length, and have their proper names accordingly; and generally into three parts in common conversation, *viz.* the head part, which reaches from the upper extremity to the place, where the rigging is fixt; the middle part, which reaches from a little below the rigging, to that place, where the lowermost part begins,

gins, and this is often called the hoist, or hoisting part; and the lowermost part, called the heel, reaches to the lower extremity. There is the same division of all the three parts; of which the mast is composed; but of the lowermost part of the mast, the heel part of it is hid below the upper deck in the body of the ship.

At the top of the main-mast, on the extremity, is fixed a piece of wood, which has a hole in it; and at the lower part of the head of the main-mast are also fixed some pieces of wood cross each other, on which lies a scaffolding called the top: through these the top-mast is thrust upwards; and when erected at its greatest height, the lower part of the top-mast, called the heel, and the upper part of the main-mast lie close to and against each other, and betwixt the top, or scaffold, and the cap: and so the top-mast and top-gallant-mast together: but the head part of the top-gallant-mast, as there is no other above it, is tapered away to a point, whereon is fixed an iron spindle and vane.

As a ship does not at all times carry her sail of the same height, but higher or lower in proportion to the strength of the wind or other circumstances, it is upon the middlemost of the three parts above-mentioned, into which the masts are divided as to length, that the sails are made to slip up and down, and are attached to the mast by several pieces of wood fixed by a rope round it to the yard, that extends the sail, and this slips up and down along with the yard, and is called the parrel.

And as it is necessary to cover these masts with some matter, that may preserve them from the weather

weather and sun, they are therefore covered with different kinds of matter, according to the uses of the different parts of the mast.

The head and heel parts of the masts are always covered with tar mixed into a consistence with lamp black, and this being frequently repeated, it forms at last a covering of the thickness of a crown, and sometimes a quarter of an inch; through which as the Sun cannot pierce, it is commonly without flaw or rent; but the middlemost part, upon which the sail slips up and down, is always rubbed with tallow, or grease, or hogs lard, to make it more slippery; and this being frequently scraped off, and anointed afresh, and in hot weather, or the summer time, it becomes extremely thin, being melted off, and frequently and in many places rubbed off by the slipping up and down of the sail and parrel, which exposes it so much, that the Sun sometimes rends it from end to end almost, and so deep as to reach the centre of the mast, and an inch wide. But as the yard, which extends the lowermost sail upon any of the masts, is commonly carried at the same height, the middlemost part of this mast is usually covered with rosin mixed with tallow or oil, and sometimes turpentine mixed with the same; and this being soft, cracks and melts with the Sun, and so leaves some places of the mast bare, and this will rend and split also like the top-mast and top-gallant-mast, that are covered with tallow or grease, but not quite so large rents.

We are now to observe, that no part of the top-gallant mast or top-mast, that was covered with the lamp black were touched with the thunder, the

greasy part only being carried away. The head of the top-gallant-mast, from the rigging upwards to the spindle, was entire, as was also its heel, for the lightening did not touch the heel, but missed the whole both of top-gallant-mast and top-mast, that lay betwixt the cap and upper end of the greasy part of the mast. Of the top-mast great pieces were carried out, of many feet in length, and 9 or 10 inches in thickness, and this on different sides of the mast, for the whole length of the greasy part. From the top of the main-mast to the upper end of that, which is covered with turpentine, there was no damage; but, from thence downwards, the cheeks were started off from the middle part, and pieces taken out winding asslant down the mast, and out of the fir part many feet in length, and 6 and 7 inches deep, and near the upper deck a piece as large as the body of a man, and 11 or 12 feet in length.

I imagine, that the vapour having insinuated itself into the rents and cracks of the mast, takes fire, and expanding itself every way, is the occasion of pieces of the mast being carried to the most distant parts of the ship; and, as a proof of this, the oak cheeks above-mentioned, though extremely well fixed to the middle part of the mast, which was of fir, by spikes and bolts, which were clinched, they having shrunk, and thereby having left the jointure a little open, had made way for the vapour to insinuate itself, which might be the occasion, that the cheeks were started off at the lower part, and the rope, wherewith they were woolded together to the middle part in four different places, with 12 turns of $2\frac{3}{4}$ inch rope in circumference, were burst all to pieces.

It must likewise be remarked, that the yards, which lay in a horizontal position, were not touched or hurt. Indeed they are always covered with lamp black and tar, as the heels and heads of the mast are, and this frequently repeated; yet they had many and large rents in them; but whether the matter, which covered them, or their horizontal position, was the reason of their escaping the thunder, is left to the learned to determine. For in this situation you have nothing to do but to sit with your arms across, and compose yourself in expectation of your fate.

I must also take notice, that no part of the rigging was hurt; for although the middle part of the top-gallant-mast, which was 18 feet long, and 9 inches diameter, was entirely burst to pieces, and carried away; yet the rigging, which surrounded the upper part, was neither burnt, scorched, nor broke. Neither did it touch the caps on the mast heads, nor the top, or round scaffolding on the mast, and in this ship it was 18 feet broad; and these as well as the yards were covered with tar and lamp black, and made of three inch deal.

I must likewise take notice, that upon the upper deck of the ship are two convenient boxes built, divided into two and into three parts, wherein are placed a lamp, which burns in the night, and a compass, whereby the ship is steered. One of these was placed very near to the main-mast in the middle of the ship, and the other close to the mizzen-mast, and both the lamps were burning at the time of the first explosion; and immediately, upon orders given, all the lights in the ship were extinguished before the bursting of the second clap; the officers imagining the inflammable

flammable vapour might be attracted by the flame of the lamp and fired thereby.

At the time of the first, I believe, there might be more than sixty men upon deck, and some of them very near the mast at the very time of the clap. Some of these were stunned and beat down; and in their arms, where they thought themselves hurt, they had a numbness, which continued some time, but not any of them otherwise hurt. Luckily before the second, the men, who were upon the quarter deck, in number about twenty, had time to retire under the auning, which is a projection of the deck of the cabin to shelter from the sun or rain; so all escaped unhurt, though sufficiently frightened. And indeed the second flash was most terrible, as it was an explosion of a great number of balls, which went off after each other, cracking like shells, which continued for the space of half a minute; and from which there was no retiring, as the door of the cabin was shut; and I believe they might have set the ship on fire, but for the great rain, which had fallen immediately before this. After this time we were in no more danger this night, the thunder being all past the ship, less frequent, and not so loud, and removing by degrees to a greater distance: and by day-light, which is here a little after five, the sky was quite cleared; a fine day; and the wind returned to the S. E. quarter.

In these cases of thunder there is not any precaution taken farther, than stopping the upper part of the pumps, because they pierce all the decks even to the outside plank in the bottom of the ship. If at sea, the sails are for the most part taken in; and

in port the men are ordered under cover, and the hatches are laid over and covered. The scuttle to the powder-room is well covered with wet swabs, and the passage secured.

Before I end this account, I shall give you the relation of an accident from thunder at Batavia.

Anno 1746. A Dutch ship, lying in the road of Batavia, having taken leave of the governor, was ready to depart for Bengal. The afternoon was calm, and towards evening they had loosed their sails, and lay ready to take up their anchor upon the coming off of the wind from the land, which is common every night. A black cloud was gathering over the hills, and the wind brought it towards the ship: by the time the cloud and the wind reached the ship, a clap of thunder burst from it just over the ship, and set fire to the main-top-sail, which being very dry, burnt with great fury; and this set fire to the rigging and mast. They immediately attempted to cut away the mast, but were hindered by the falling of the rigging, which was burnt, from the head of the mast. By degrees the fire communicated to the other masts, and obliged the people to desert the ship; and afterwards it took hold of the body of the ship, and burning down to the powder, the upper part of the hull blew up, and the bottom part sunk in the place, where she was at anchor.

Anno 1741. Bencoolen road on the S. W. side of the Island of Sumatra, Lat. 4° 0' South. There lay here two ships, one an European, the other a country trading ship, both belonging to the East India

India Company. Here, as well as in the streight of Malacca, you have periodical winds, which blow for six months of the year from the same quarter of the horizon, and the other six months from the opposite quarter; and it is observable, that these thunder-showers and squalls of wind usually come contrary to these stated winds, which are calmed during the thunder, but return to their constant quarter as soon as the thunder and rain are past. If I recollect aright, in the above year 1741, in June, the weather was very hot and sultry, and the constant wind but very faint. The wind came after this from the land, and almost opposite to the usual point a very faint air; and the thunder was frequent and close to the ships, which lay near each other, but the fog and rain prevented their seeing each other; but they often trembled and shook by the explosion of the thunder. One of these claps burst upon the country ship, which by this time had her top-masts struck; that is, lowered down along the lower-masts. This clap carried away and burst to pieces all the part of the lower-mast from where the yard is carried aloft to within six or seven feet of the upper deck. The mast was woolded with ropes of 2 $\frac{1}{2}$ size in different places, which were burst asunder at every turn of it; and the mast all shivered into small splinters, and mostly carried overboard. Here also the main-mast was made of fir, and the part, which was split and shivered to pieces, was the part usually coated with turpentine mixed as before-said with tallow or oil: and the main-top-mast, which was made of a wood of the country called teak, and is of a texture like to oak, but stronger, was untouched, notwithstanding

it lay parallel, and touched the mast for the whole length of the part carried away. Here the vapour must be very low, being wholly below the top, which was unhurt, as was all the rigging and yards of this particular mast. And from the upper deck of the ship to the top could not, in this ship, which was but small, exceed 42 or 44 feet. And in this last case the explosion must have been like gunpowder endeavouring to expand itself every way, because the top and top-mast, and rigging, which was above it, sunk perpendicularly down on the body of the ship, as did the top-mast also, which pierced the upper deck, and stood upright. In this ship there might be at the time of the explosion seventy men upon deck, and not one of them hurt; which I imagine was owing to the vapour being distant above the deck more than the height of a man, as was apparent from a remnant of the mast, which was not touched for six or seven feet immediately above the deck.

The relater was an eye-witness to both these accidents. In this last there remained a stinking vapour for some time; but in the first case there was not the least smell of sulphur, or any other thing.

Robert Veitch.

LII. *A Demonstration of the Second Rule in the Essay towards the Solution of a Problem in the Doctrine of Chances, published in the Philosophical Transactions, Vol. LIII. Communicated by the Rev. Mr. Richard Price, in a Letter to Mr. John Canton, M. A. F. R. S.*

Dear Sir,

Nov. 26, 1764.

Read Dec. 6, 1764. **I** Send you the following *Supplement to the Essay on a Problem in the Doctrine of Chances*, hoping that you may not think it improper to be communicated to the Royal Society. I should not have troubled you again in this way had I not found that some additions to my former papers were necessary in order to explain some passages in them, and particularly what is hinted in the note at the end of the Appendix. "I have first given the deduction of Mr. Bayes's second rule chiefly in his own words; and then added, as briefly as possible, the demonstrations of several propositions, which seem to improve considerably the solution of the problem, and to throw light on the nature of the curve by the quadrature of which this solution is obtained." Perhaps, there is no reason for being very anxious about proceeding to further improvements. It would, however, be very agreeable to me to see a yet easier and nearer approximation to the value of the two series's in the first rule: but this I must leave abler persons to seek, chusing now entirely to drop this subject.

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The solution of the problem enquired after in the papers I have sent you has, I think, been hitherto a *desideratum* in philosophy of some consequence. To this we are now in a great measure helped by the abilities and skill of our late worthy friend; and thus are furnished with a necessary guide in determining the nature and proportions of unknown causes from their effects, and an effectual guard against one great danger to which philosophers are subject; I mean, the danger of founding conclusions on an insufficient induction, and of receiving just conclusions with more assurance than the number of experiments will warrant. I am, under a sense of the value of your friendship, heartily yours,

Richard Price.

C *f* has moved till it coincides with D *b* and A C *f* becomes A D *b*. In like manner, from Art. 3. in the Essay, it appears that the ratio of H D *b* to H O is

$$\frac{p}{q+1} \times \frac{q+1}{q+2} \times \frac{p}{p} + \frac{p}{q+2} \times \frac{q}{p} + \frac{p}{q+2} \times \frac{p-1}{q+3} \times \frac{q}{p^2} + \mathcal{C}.$$

From hence it follows that the ratio of the difference between A D *b* and H D *b* to H O is $\frac{p}{n} \times \frac{q}{ab}$ multiplied by

$$\text{the difference between the series } \frac{p}{p+1} + \frac{q}{p+1} \times \frac{p^2}{pq+2q} + \frac{q \times q-1 \times p^3}{p+1 \times p+2 \times pq^2+3q^2} + \mathcal{C}.$$

$$\text{and the series } \frac{q}{q+1} + \frac{p \times q^2}{q+1 \times pq+2p} + \frac{p \times p-1 \times q^3}{q+1 \times q+2 \times p^2q+3p^2} + \mathcal{C}.$$

the former series being to be subtracted from the latter, if H D *b* is greater than A D *b*, and *vice versa*.

2. The ratio of any term in the former of the two foregoing series to that which next but one follows the correspondent term in the latter is $\frac{pq+p}{p \times q} \times$

$$\frac{pq+2p}{p \times q} \times \frac{p \times q}{q \times p+q} \times \frac{pq+3p}{pq-q} \times \frac{pq}{pq+2q} \times \frac{pq+4p}{pq-2q} \times \frac{pq-p}{pq+3q} \times \frac{pq+5p}{pq-3q} \times \frac{pq-2p}{pq+4q} \times \frac{pq+6p}{pq-4q} \mathcal{C}.$$

taking twice as many terms and four over as there are units in the number which expresses the distance of the term in the former series from its first term; which

ratio if q be greater than p , it is evident must be greater than the ratio of equality. Wherefore, if from the second series you subtract the two first terms which together are less than two, the remainder is less than the former series; and of consequence, the former series subtracted from the latter cannot leave a remainder so great as two. And therefore in this case, that is, when q is greater than p , by the preceding article, the ratio of $HD b - AD b$ to HO cannot be so great as $\frac{p^q}{2ab}$.

3. The curve ADH being as before divided into two parts $AD b$ and $HD b$, let the ordinates Cf and Ft be placed on each side of $D b$ and at the same distance from it, and let z be the ratio of $b f$ or $b t$ to AH . Then if y , x and r be respectively the ratios of Cf , Af and Hf to AH , by the nature of the curve $y = x^{\frac{p}{q}}$. But because the ratio of $A b$ to AH is a , and that of $b f$ to AH is z , the ratio of $A b - b f (= Af)$ to AH is $a - z$. Wherefore $a - z = x$. And in like manner $b + z = r$. But $y = x^{\frac{p}{q}}$, and y is the ratio of Cf to AH . Wherefore the ratio of Cf to AH is $\overline{a - z}^{\frac{p}{q}} \times \overline{b + z}^{\frac{q}{q}}$. And in like manner the ratio of Ft to AH is $\overline{a + z}^{\frac{p}{q}} \times \overline{b - z}^{\frac{q}{q}}$. And consequently Cf is to Ft as $\overline{a - z}^{\frac{p}{q}} \times \overline{b + z}^{\frac{q}{q}}$ is to $\overline{a + z}^{\frac{p}{q}} \times \overline{b - z}^{\frac{q}{q}}$.

4. If q is greater than p , $\overline{a + z}^{\frac{p}{q}} \times \overline{b - z}^{\frac{q}{q}}$ is greater than $\overline{a - z}^{\frac{p}{q}} \times \overline{b + z}^{\frac{q}{q}}$, and the ratio between them increases as z increases. For the hyperbolic logarithm of

of that ratio taken as usual, and then instead of p and q putting na and nb because $a = \frac{p}{n}$ and $b = \frac{q}{n}$ (Vid. Art. 1.) you will find to be $2n$ multiplied by the series $\frac{b^2 - a^2}{3b^2a^2} \times x^3 + \frac{b^4 - a^4}{5b^4a^4} \times x^5 + \frac{b^6 - a^6}{7b^6a^6} \times x^7 \&c.$ which logarithm when q is greater than p , and therefore b greater than a has all its terms positive, and so much the greater as x is greater; and therefore it is the logarithm of a ratio greater than that of equality, and which increases as x increases.

5. By Art. 3. Ft is to Cf as $\overline{a+x}^p \times \overline{b-x}^q$ is to $\overline{a-x}^p \times \overline{b+x}^q$. And by Art. 4. $\overline{a+x}^p \times \overline{b-x}^q$ is greater than $\overline{a-x}^p \times \overline{b+x}^q$, and the ratio between them increases as x increases, if q is greater than p . Wherefore, upon this supposition, also Ft is greater than Cf , and the ratio between them increases as x or bt and bf increases, and consequently this will be true also concerning the areas described by them as their equal abscisses bt and bf increase. Wherefore, when q is greater than p , $DbtF$ is greater than $DbfC$, and the ratio between them increases as $bf = bt$ increases.

6. Because Ab is to Hb as p is to q , when q is greater than p , Hb is greater than Ab . In Hb therefore taking bl equal to Ab , by the preceding Art. the part of the figure HDb which insits upon bl will be greater than ADb , and the ratio of that part of HDb to ADb will be greater than the ratio of $DbtF$ to $DbfC$. Consequently, much more (q being greater than p) the whole figure HDb is greater.

greater than ADb , and the ratio of HDb to ADb is greater than that of $DbtF$ to $DbfC$.

7. When q is greater than p , $1 - \frac{n z}{p q} \Big| z$ is greater than $1 - \frac{n z}{p} \Big|^p \times 1 + \frac{n z}{q}$ and less than $1 - \frac{n z}{q} \Big|^q \times 1 + \frac{n z}{p} \Big|^p$. For the fluxion of $1 - \frac{n z}{p q}$ is $\frac{n^3 z \dot{z}}{p q} \times 1 - \frac{n^2 z^2}{p q} \Big|^{\frac{n}{z}-1}$ and the fluxion of $1 - \frac{n z}{p} \Big|^p \times 1 + \frac{n z}{q} \Big|^q$ (because $p + q = n$) is $-\frac{n^3 z \dot{z}}{p q} \times 1 - \frac{n z}{p} \Big|^{p-1} \times 1 + \frac{n z}{q} \Big|^{q-1}$. Wherefore $1 - \frac{n^2 z^2}{p q} \Big|^{\frac{n}{z}}$ is to $1 - \frac{n z}{p} \Big|^p \times 1 + \frac{n z}{q} \Big|^q$ as the fluxion of the former multiplied by $1 - \frac{n^2 z^2}{p q} \Big|^{\frac{n}{z}}$ to the fluxion of the latter multiplied by $(1 - \frac{n z}{p} \Big|^p \times 1 + \frac{n z}{q} \Big|^q)$ or $1 - \frac{n z}{p} + \frac{n z}{q} - \frac{n z}{p q}$. From which analogy, because q is greater than p , it is plain that $1 - \frac{n z}{p} \Big|^p \times 1 + \frac{n z}{q} \Big|^q$ varies at a greater rate in respect of its own magnitude than $1 - \frac{n^2 z^2}{p q} \Big|^{\frac{n}{z}}$ does. And, because their fluxions as found out before have a negative sign before them, they both decrease as z increases;

creases; consequently, if they are ever equal, as z increases the latter must be the largest. But when $z = 0$ they are each equal to 1. In like manner the other part of this article appears. And hence, since $a = \frac{p}{q}$ and $b = \frac{q}{p}$, it is manifest that $a^p b^q \times 1 - \frac{z^2}{p q} \Big|^\frac{n}{2}$ is greater than $\overline{a - z}^p \times \overline{b + z}^q$ and less than $\overline{a + z}^p \times \overline{b - z}^q$, when q is greater than p .

8. Suppose now further that the curve $R Q W$ be described meeting the lines $D b$, $F t$, $b t$ produced in R , Q , W , in such manner that $F t$, which is to $C f$ as $\overline{a + z}^p \times \overline{b - z}^q$ to $\overline{a - z}^p \times \overline{b + z}^q$ (Vid. Art. 3.) shall be to $Q t$ as $\overline{a + z}^p \times \overline{b - z}^q$ to $a^p b^q \times 1 - \frac{z^2}{p q} \Big|^\frac{n}{2}$ wherever the points t and f fall at equal distances from b . And it is manifest by the foregoing Art. that $Q t$ must always be greater than $C f$, and less than $F t$. And of consequence the same must be true concerning the areas described by their motion while their equal abscisses increase. Wherefore $R b t Q$ is greater than $D b f C$, and less than $D b t F$.

9. Since $F t$ is to $Q t$ as $\overline{a + z}^p \times \overline{b - z}^q$ to $a^p b^q \times 1 - \frac{z^2}{p q} \Big|^\frac{n}{2}$; and $\overline{a + z}^p \times \overline{b - z}^q$ (by Art.

Art. 3.) expresses the ratio of Ft to AH ; the ratio of Qt to AH must be $a^p b^q \times \sqrt[n]{1 - \frac{n^2 z^2}{pq}}^n$, and as has been all along supposed z is the ratio of bt to AH . Wherefore, by squaring the curve $RbtQ$, it will appear that the ratio of $RbtQ$ to HO is

$$\begin{aligned} & a^p b^q \times z - \frac{n^3 z^3}{2 \cdot 3 p q} + \frac{n-2}{4} \times \frac{n^5 z^5}{2 \cdot 5 p^2 q^2} - \frac{n-2}{4} \times \\ & \frac{n-4}{6} \times \frac{n^7 z^7}{2 \cdot 7 p^3 q^3} + \mathcal{E}c. \text{ which (if } m^2 = \frac{n^2}{2 p q} \text{) is} \\ & a^p b^q \times \frac{\sqrt{2 p q}}{n \sqrt{n}} \times m z - \frac{m^3 z^3}{3} + \frac{n-2}{2 n} \times \frac{m^5 z^5}{5} - \frac{n-2}{2 n} \\ & \times \frac{n-4}{3 n} \times \frac{m^7 z^7}{7} + \frac{n-2}{2 n} \times \frac{n-4}{3 n} \times \frac{n-6}{4 n} \times \frac{m^9 z^9}{9} \\ & - \mathcal{E}c. \text{ Which last series when } \frac{n^2 z^2}{p q} = 1, \text{ and con-} \\ & \text{sequently the ordinate } Qt \text{ vanishes, becomes } B - \\ & \frac{B^3}{3} + \frac{B^2-1}{2 B^2} \times \frac{B^5}{5} - \frac{B^2-1}{2 B^2} \times \frac{B^2-1}{3 B^2} \times \frac{B^7}{7} + \mathcal{E}c. \\ & \text{taking } B^2 = \frac{n^2}{2} \end{aligned}$$

10. If $B^2 = \frac{n^2}{2}$ the ratio of the whole figure $RQWb$ to HO is $\frac{\sqrt{2 p q}}{n \sqrt{n}} \times a^p b^q \times B - \frac{B^3}{3} + \frac{B^2-1}{2 B^2} \times \frac{B^5}{5} - \mathcal{E}c.$ Now, (by Prop. 10. Art. 4. of the Essay) the ratio of $ACFH$ to HO is $\frac{1}{n+1}$

$\times \frac{1}{E}$, E being the coefficient of that term of the binomial $\overline{a+b}^n$ expanded in which occurs $a^p b^q$. Wherefore, the ratio of $R Q W b$ to $A C F H$ is $\frac{n+1}{n} \times \frac{\sqrt{2pq}}{\sqrt{n}} \times E a^p b^q \times B - \frac{B^3}{3} + \frac{B^2-1}{2B^2} \times \frac{B^3}{5} \mathcal{C}.$ Put G now for the coefficient of the middle term of the same binomial, and if $p = q = \frac{n}{2}$, $E = G$, $a = \frac{1}{2} = b$ the area $R Q W b$ is equal to half $A C F H$; for then $Q t$, $F t$, $C f$ are all equal, and consequently the areas $R Q W b$, $H D b$ and $A D b$. Wherefore, the

series $B - \frac{B^3}{3} + \mathcal{C}.$ is equal to $\frac{\sqrt{2n}}{n+1} \times \frac{2^{n-1}}{G}$. But the series $B - \frac{B^3}{3} + \mathcal{C}.$ (because $B^2 = \frac{n}{2}$) does not alter whatever p and q are, whilst their sum n remains the same. Wherefore, in all cases, the ratio of $R Q W b$ to $A C F H$ is $\frac{\sqrt{pq}}{n} \times \frac{E a^p b^q}{G} \times 2^n$.

11. By Prop. 10. Art. 4. of the Essay, the ratio of $A C F H$ to $H O^*$ is $\frac{1}{n+1} \times \frac{1}{E}$; and by Art. 9. the ratio of $R b t Q$ to $H O$ is $a^p b^q \times \frac{\sqrt{2pq}}{n\sqrt{n}} \times m \times - \frac{m^3 z^3}{3} + \frac{n-2}{2n} \times \frac{m^3 z^3}{5} \mathcal{C}.$ Wherefore, the ratio of

* It is hoped that the imperfection of the figure all along referred to will be excused. The lines $R b$ and $D b$ should appear equal; and it will be found presently, that the curve line $A C D F H$ should have been drawn from F and C convex towards $A H$.

R b t Q to A C F H is $\frac{n+1}{n} \times \frac{\sqrt{2pq}}{\sqrt{n}} \times E a^p b^q \times m z - \frac{m^3 z^3}{3}$
 $+ \frac{n-2}{2n} \times \frac{m^5 z^5}{5} - \frac{n-2}{2n} \times \frac{n-4}{3n} \times \frac{m^7 z^7}{7} + \mathcal{E}c.$
 Likewise, by Art. 10. the ratio of R Q W b to
 A C F H is $\frac{\sqrt{pq}}{n} \times \frac{E a^p b^q}{G} \times 2^n$. Wherefore the ra-
 tio of R b t Q to R Q W b is $\frac{n+1}{\sqrt{n}} \times \frac{\sqrt{2}}{2^n} \times G$
 $\times m z - \frac{m^3 z^3}{3} + \mathcal{E}c.$

12. By Art. 2. 6. When q is greater than p , the
 ratio of H D b — A D b to H O is less than
 $\frac{2 a^p b^q}{n}$. And by Prop. 10. Art. 4. of the Essay, the
 ratio of A C F H or H D b + A D b to H O is
 $\frac{1}{n+1} \times \frac{1}{E}$. Wherefore, the sum of these two ratios,
 or the ratio of 2 H D b to H O, is less than $\frac{1}{n+1}$

$\times \frac{1}{E} + \frac{2 a^p b^q}{n}$; and the difference between them,
 or the ratio of 2 A D b to H O is greater than
 $\frac{1}{n+1} \times \frac{1}{E} - \frac{2 a^p b^q}{n}$. Wherefore, the ratio of 2 H D b
 to 2 A D b, or that of H D b to A D b, is less than
 that of $\frac{1}{n+1} \times \frac{1}{E} + \frac{2 a^p b^q}{n}$ to $\frac{1}{n+1} \times \frac{1}{E} - \frac{2 a^p b^q}{n}$, which
 is equal to the ratio of $1 \times 2 E a^p b^q + \frac{2 E a^p b^q}{n}$ to 1

$- 2 E a^p b^q - \frac{2 E a^p b^q}{n}$. But the ratio of $H D b$ to $A D b$, by Art. 6. is greater than the ratio of $D b t F$ to $D b f C$, when q is greater than p . Wherefore, much more when q is greater than p , the ratio of $D b t F$ to $D b f C$ will be less than that of $1 + \frac{2 E a^p b^q}{n}$ to $1 - 2 E a^p b^q - \frac{2 E a^p b^q}{n}$. And because, by Art. 8. $R b t Q$ is a mean between $D b t F$ and $D b f C$, the ratio of $D b t F$ to $R b t Q$ will be less than that of $1 + \frac{2 E a^p b^q}{n}$ to $1 - 2 E a^p b^q - \frac{2 E a^p b^q}{n}$. And the ratio of $D b f C$ to $R b t Q$ will be greater than that of $1 - 2 E a^p b^q - \frac{2 E a^p b^q}{n}$ to $1 + \frac{2 E a^p b^q}{n}$.

R U L E II.

If nothing is known of an event but that it has happened p times and failed q in $p + q$ or n trials, and q be greater than p ; and from hence I judge that the probability of its happening in a single trial lies between $\frac{p}{n}$ and $\frac{p}{n} + x$, (if $m^2 = \frac{n^3}{2 p q}$, $a = \frac{p}{n}$, $b = \frac{q}{n}$, E the coefficient of the term in which occurs $a^p b^q$ when $(a + b)^n$ is expanded, and $\Sigma =$

$$\frac{n+1}{n} \times \frac{\sqrt{2 p q}}{\sqrt{n}} \times E a^p b^q \times m x - \frac{m^3 x^3}{3} + \frac{n-2}{2 n} \times \frac{m^5 x^5}{5}$$

$\frac{n-2}{2n} \times \frac{n-4}{3n} \times \frac{m'z'}{7} + \mathcal{E}c.)$ my chance to be in the right is greater than Σ , and less than $\Sigma \times$

$$\frac{1 + 2E \frac{p}{a} \frac{q}{b} + \frac{2E \frac{p}{a} \frac{q}{b}}{n}}{1 - 2E \frac{p}{a} \frac{q}{b} - \frac{2E \frac{p}{a} \frac{q}{b}}{n}}.$$
 For by Art. 11. com-

pared with the value of Σ here set down, the ratio of $RbtQ$ to $ACFH$ is Σ . But by Art. 8. $DbtF$ is greater than $RbtQ$, and by Art. 12. the ratio of $DbtF$ to $RbtQ$ is less than that of $1 + 2E \frac{p}{a} \frac{q}{b} + \frac{2E \frac{p}{a} \frac{q}{b}}{n}$ to $1 - 2E \frac{p}{a} \frac{q}{b} - \frac{2E \frac{p}{a} \frac{q}{b}}{n}$. From whence

it is plain that the ratio of $DbtF$ to $ACFH$ is greater than Σ , and less than $\Sigma \times \frac{1 + 2E \frac{p}{a} \frac{q}{b} + \frac{2E \frac{p}{a} \frac{q}{b}}{n}}{1 - 2E \frac{p}{a} \frac{q}{b} - \frac{2E \frac{p}{a} \frac{q}{b}}{n}}.$

But, as appears from the 10th Proposition in the Essay, the chance that the probability of the event lies between

$\frac{p}{n}$ and $\frac{p}{n} + z$ (that is, between the ratio of Ab to AH , and that of At to AH) is the ratio of $DbtF$ to $ACFH$. Wherefore, the chance I am right in my guess is greater than Σ and less than $\Sigma \times$

$$\frac{1 + 2E \frac{p}{a} \frac{q}{b} + \frac{2E \frac{p}{a} \frac{q}{b}}{n}}{1 - 2E \frac{p}{a} \frac{q}{b} - \frac{2E \frac{p}{a} \frac{q}{b}}{n}}.$$

In like manner, 2dly, the same things supposed, if I judge that the probability of the event lies between $\frac{p}{n}$ and $\frac{p}{n} - z$, my chance to be right is less than Σ ,

and greater than $\Sigma \times \frac{1 - 2 E a^p b^q - \frac{2 E a^p b^q}{n}}{1 + 2 E a^p b^q + \frac{2 E a^p b^q}{n}}$. This

is manifest as the other case, because $D b f C$ is less than $R b t Q$, but the ratio between them is greater

than that of $1 - 2 E a^p b^q - \frac{2 E a^p b^q}{n}$ to $1 + 2 E a^p b^q + \frac{2 E a^p b^q}{n}$

3dly, If, other things supposed as before, p is greater than q , and I judge the probability of the event lies between $\frac{p}{n}$ and $\frac{p}{n} + z$, my chance to be right is less than Σ , and greater than $\Sigma \times$

$1 - 2 E a^p b^q - \frac{2 E a^p b^q}{n}$. But if I judge it lies between $\frac{p}{n}$ and $\frac{p}{n} - z$, my chance is greater than Σ , and

less than $\Sigma \times \frac{1 + 2 E a^p b^q + \frac{2 E a^p b^q}{n}}{1 - 2 E a^p b^q - \frac{2 E a^p b^q}{n}}$. And if $p = q$,

which

which ever of these ways I guess, my chance is Σ exactly. This may be proved in the same manner with the foregoing cases, where q is greater than p , or may be proved from them by considering the happening and failing of an event, as the same with the failing and happening of its contrary.

4thly, Other things supposed the same, whether q be greater or less than p , and I judge that the probability of the event lies between $\frac{p}{n} + z$ and $\frac{p}{n} - z$, my chance is greater than $\frac{2 \Sigma}{1 + 2 E a^p b^q + 2 \frac{E a^p b^q}{n}}$, and

less than $\frac{2 \Sigma}{1 - 2 E a^p b^q - 2 \frac{E a^p b^q}{n}}$. This is an evi-

dent corollary from the cases already determined. And here, if $p = q$, my chance is 2Σ exactly.

Thus far I have transcribed Mr. Bayes.

It appears, from the Appendix to the Essay, that the rule here demonstrated, though of great use, does not give the required chance within limits sufficiently narrow. It is therefore necessary to look out for a contraction of these limits; and this, I think, we shall discover by the help of the following deductions; which, for the sake of greater distinctness, I shall give as a continuation of the foregoing Articles.

13. The ratio of the fluxion of $\sqrt[p]{1 - \frac{n^2 z^2}{pq}}$ to the

$$\text{fluxion of } \sqrt[p]{1 + \frac{nz}{p}} \times \sqrt[q]{1 - \frac{nz}{q}} \text{ is } \frac{1 - \frac{n^2 z^2}{pq}}{\sqrt[p-1]{1 + \frac{nz}{p}} \times \sqrt[q-1]{1 - \frac{nz}{q}}}$$

$$\text{and the ratio of the fluxion of } \sqrt[p]{1 - \frac{nz}{p}} \times \sqrt[q]{1 + \frac{nz}{q}}$$

$$\text{to the fluxion of } \sqrt[p]{1 - \frac{n^2 z^2}{pq}} \text{ is } \frac{\sqrt[p-1]{1 - \frac{nz}{p}} \times \sqrt[q-1]{1 + \frac{nz}{q}}}{\sqrt[p]{1 - \frac{n^2 z^2}{pq}}}$$

This will immediately appear from Art. 7.

14. While z is increasing from nothing till $\frac{n^2 z^2}{pq}$ becomes equal to unity, these two ratios are at first greater than the ratio of equality, and increase as z increases, till they come to a *maximum*. Afterwards they decrease untill they become first equal to the ratio of equality, and then less. This is proved by finding the hyperbolic logarithms of these ratios. The hyperbolic logarithm

$$\text{of the first is the series } \frac{q-1}{q} - \frac{p-1}{p} \times \frac{nz}{p} +$$

$$\frac{q-1}{q^2} + \frac{p-1}{p^2} - \frac{n-2}{pq} \times \frac{n^2 z^2}{2} + \frac{q-1}{q^3} - \frac{p-1}{p^3} \times$$

$$\frac{z^3}{3} + \frac{q-1}{q^4} + \frac{p-1}{p^4} - \frac{n-2}{p^2 q^2} \times \frac{1}{4} + \frac{1}{q}$$

$-\frac{p-1}{p^5} \times \frac{n^5 z^5}{5} + \frac{q-1}{q^6} + \frac{p-1}{p^6} - \frac{n-2}{p^3 q^3} \times \frac{n^6 z^6}{6}$
 $+ \&c.$ The hyperbolic logarithm of the second

ratio is the series $\frac{q-1}{q} - \frac{p-1}{p} \times n z - \frac{q-1}{q^2} +$
 $\frac{p-1}{p^2} - \frac{n-2}{p q} \times \frac{n^2 z^2}{2} + \frac{q-1}{q^3} - \frac{p-1}{p^3} \times \frac{n^3 z^3}{3}$

$\frac{q-1}{q^4} + \frac{p-1}{p^4} - \frac{n-2}{p^2 q^2} \times \frac{n^4 z^4}{4} + \&c.$ It will appear from examining these two serieses (q all along supposed greater than p) that while z is small the value of each of them is positive, and increases as z increases till it becomes a *maximum*, after which it decreases till it becomes nothing, and after that negative; which demonstrates this article.

15. The former of the two ratios in Art. 13. (q being greater than p) is at first, while z is increasing from nothing, less than the second ratio; and does not become equal to it, till some time after both ratios have been the greatest possible.

Upon considering the two serieses in the last Art. it will appear that the first term of the first series is always positive, the second negative, the third also negative, after which the terms become alternately positive and negative. On the other hand, it will appear that in the second series the two first terms are always positive, and all that follow negative. But as the serieses converge very fast when z is small, the second term being negative in the first series and positive in the second, has a greater effect in making the first series less than the second, than can be compensated for by the terms being afterwards alternately negative and positive

positive in the one, and all negative in the other. It will further appear from considering these serieses, that the first must continue less than the second 'till z becomes so large as to make the fourth term equal to the second, in which circumstances the two serieses are nearly equal. Afterwards, as z goes on to increase, the value of both lessens continually; but the second now decreasing fastest, as before it increased fastest, becomes first nothing. After which, the other series becomes nothing; and after that both remain negative. From hence it is easy to infer this Article.

16. What has been now shewn of the ratio of the fluxion of $\sqrt[p]{1 - \frac{n^2 z^2}{pq}}$ to the fluxion of $\sqrt[p]{1 + \frac{n z}{p}}$ \times $\sqrt[p]{1 - \frac{n z}{q}}$ compared with the ratio of the fluxion of $\sqrt[p]{1 - \frac{n z}{p}}$ \times $\sqrt[p]{1 + \frac{n z}{q}}$ to the fluxion of $\sqrt[p]{1 - \frac{n^2 z^2}{pq}}$ is also true of the ratio of the fluxion of $a^p b^q \times$

$\sqrt[p]{1 - \frac{n z}{q}}$ (or Qt in the figure) to the fluxion of $\sqrt[p]{a + z}^p \times \sqrt[q]{b - z}^q$ (or Ft) compared with the ratio of the fluxion of $\sqrt[p]{a - z}^p \times \sqrt[q]{b + z}^q$ (or Cf) to the fluxion of $a^p b^q \times \sqrt[p]{1 - \frac{n^2 z^2}{pq}}$ or Qt ; the latter quantities being only the former multiplied by the common and permanent quantity $a^p b^q$. It appears, therefore, that if we conceive Ft , Qt , Cf (Vid. Vol. LIV. S f Fig.)

Fig.) to move with equal and uniform velocities, from $D b$ and $R b$ along AH , in order to generate the areas $H D b$, $R W b$, $A D b$; $C f$ will at first not only decrease faster than $Q t$, and $Q t$ than $F t$; but the ratio of the rate at which $C f$ decreases to the rate at which $Q t$ decreases, will be greater than the ratio of the rate at which $Q t$ decreases to the rate at which $F t$ decreases. It appears also that after some time, first $C f$ and $Q t$, and then $Q t$ and $F t$ will come to decrease equally; after which, $Q t$ will decrease faster than $C f$, and $F t$ faster than $Q t$.

17. The curves DFH , RQW , DCA , have each of them a point of contrary flexure; and the value of z , or of the equal abscissæ at that point, is in all three $\frac{\sqrt{\frac{p q}{n^3 - n^2}}}{\sqrt{\frac{p q}{n^3 - n^2}}}$. This may be found in the common manner, by putting the second fluxions of the ordinates equal to nothing. In the single case, when either p or q is equal to unity, one of these points vanishes, or coincides with A or H .

18. At the points of contrary flexure (q being greater than p) the ratio of the fluxion of $Q t$ to the fluxion of $F t$ is a *maximum*; and the same is true of the ratio of the fluxion of $C f$ to the fluxion of $Q t$. This is found by making the fluxions of the logarithms of these ratios, or of

$$\frac{p q}{1 + \frac{n z}{p}} \times \frac{p-1}{1 - \frac{n z}{q}}^{q-1}, \text{ and } \frac{1}{1 - \frac{n^2 n^2}{p q}} \times \frac{1}{1 + \frac{n z}{q}}^{q-1}$$

equal

equal to nothing: which will give the value of z equal to $\frac{\sqrt{pq}}{\sqrt{n^3 - n^2}}$, or the same with the value of z at the points of contrary flexure.

19. At the points of contrary flexure, the ratio of the fluxion of Cf to the fluxion of Qt , is greatest in comparison of the ratio of the fluxion of Qt to the fluxion of Ft . This is proved by finding the value of z when the fluxion of the former ratio

$$\frac{\left(1 - \frac{z^2 z^2}{p^2}\right)^{p-1} \times \left(1 - \frac{n^2 z^2}{a^2}\right)^{q-1}}{1 - \frac{n^2 z^2}{p^2}}$$

divided by the latter, or of

$$1 - \frac{n^2 z^2}{p^2}$$

is nothing, which will still give $z = \frac{\sqrt{pq}}{\sqrt{n^3 - n^2}}$. The reason, therefore, in the nature of the curve, which, as the ordinates flow, keeps at first the excess of Ft above Qt less than the excess of Qt above Cf , operates with its greatest force at the points of contrary flexure.

20. The greatest part of the area $RQWb$ lies between Rb , and the ordinate at the point of contrary flexure. By Art. 11 the ratio of $RbtQ$ to $RQWb$ is

$$\frac{n+1}{\sqrt{n}} \times \frac{\sqrt{2}}{2^n} \times G \times \frac{mz - \frac{m^3 z^3}{3}}{3} + \frac{n-2}{2n} \times \frac{m^5 z^5}{5} -$$

Ec. Substitute here $\sqrt{\frac{pq}{n^3 - n^2}}$ for z , and $\frac{2^n}{\sqrt{nK} \times H}$

* for G (K being the ratio of the quadrantal arc to

* This is always the true value of G ; but it would be too tedious to give the demonstration of this here.

radius, and H the ratio whose hyperbolic logarithm is $\frac{3}{12n} - \frac{15}{360n^3} + \frac{63}{1260n^5} * \&c.$) and the ratio of $RbtQ$ to $RQWb$ at the point of contrary flex-

$$\text{ure, will be } \frac{n+1}{\sqrt{n} \times \sqrt{n-1}} \times \frac{.797884}{H} \times 1 - \frac{n}{2 \cdot 3 \cdot n-1} + \frac{n \times n-2}{2 \cdot 5 \cdot 4 \cdot n-1^2} - \frac{n \cdot n-2 \cdot n-4}{2 \cdot 3 \cdot 7 \cdot 8 \cdot n-1^3} + \frac{n \cdot n-2 \cdot n-4 \cdot n-6}{2 \cdot 3 \cdot 4 \cdot 9 \cdot 16 \cdot n-1^4} - \&c.$$

Now when n is little, the value of this expression will be considerably greater than .6822. It approaches to this continually as n increases; and when n is large, it may be taken for this exactly. Thus when $n = 6$, this expression is equal to .804. When $n = 110$, it is equal to .6903. If we would know the ratio of $RbtQ$ to $RQWb$, when Cf comes to decrease no faster in respect of Qt , than Qt decreases in respect of Ft ; that is, when the excess of Qt above Cf , is greatest in comparison of the excess of Ft above Qt , it may be found (by putting the fourth term of the series in the 14th Art. equal to the second term, and then finding the value of x) to be about .8426, when n , p , and q are considerable; and in other cases greater.

Coroll. 'Tis easy to gather from hence that in like manner the greatest part of the area ADH lies between the two ordinates at the points of contrary flexure †.

* Vid. the Second Rule in the Essay, Phil. Transf. Vol. LIII.

† From this Article may be inferred a method of finding at once, without any labour, whereabouts it is reasonable to judge the probability of an unknown event lies, about which a given number of experiments have been made. For when

21. *RbtQ* is greater than the arithmetical mean between *DbtF* and *DbfC*. This appears from the latter part of Art. 19. for what is there proved of the ordinates must hold true of the contemporary areas generated by them. And though beyond the points at which the ratio of the decrease of *Qt* to the decrease of *Ft* comes to an equality with the ratio of the decrease of *Qt* to the decrease of *Cf*, the excess of *Ft* above *Qt* begins to grow larger than before in respect of the excess of *Qt* above *Cf*; yet as it appears from the last article, that above five fixths of the areas *RQWb* and *ACFH* are generated before the ordinates come to these points, and as also beyond these points the said ratios, 'till they become

neither *p* nor *q* are very small, or even not less than 10, it will be nearly an equal chance, that the probability of the event lies

between $\frac{p}{n} + \frac{\sqrt{pq}}{\sqrt{2n^3 - 2n^2}}$ and $\frac{p}{n} - \frac{\sqrt{pq}}{\sqrt{2n^3 - 2n^2}}$. It will be

the odds of two to one that it lies between $\frac{p}{n} + \frac{\sqrt{pq}}{\sqrt{2n^3 - 2n^2}}$

and $\frac{p}{n} - \frac{\sqrt{pq}}{\sqrt{2n^3 - 2n^2}}$; and the odds of five to one that it lies

between $\frac{p}{n} + \frac{\sqrt{2pq}}{\sqrt{2n^3 - 2n^2}}$ and $\frac{p}{n} - \frac{\sqrt{2pq}}{\sqrt{2n^3 - 2n^2}}$. For in-

stance; when *p* = 1000, *q* = 100, there will be nearly an equal chance, that the probability of the event lies between $\frac{10}{11} + \frac{1}{163}$

and $\frac{10}{11} - \frac{1}{163}$; two to one that it lies between $\frac{10}{11} + \frac{1}{115}$ and

$\frac{10}{11} - \frac{1}{115}$; and five to one that it lies between $\frac{10}{11} + \frac{1}{81}$ and

$\frac{10}{11} - \frac{1}{81}$.

negative

negative and for some time afterwards, are but small; the effect produced before towards rendering the excess of $D b t F$ above $R b t Q$ always less than the excess of $R b t Q$ above $D b f C$, will be such as cannot be compensated for afterwards.

A further proof of this will appear from considering that even when $R b t Q$ is increased to $R Q W b$, it is but little short of the arithmetical mean between $A D b$ and $H D b$. For from Art. 11. and 20. it may be inferred that the ratio of the whole area $R Q W b$ to this mean, or to $\frac{A C F H}{2}$, is $b \times H$, which is never far from the ratio of equality, but when both p and q are of any considerable magnitude, it is very nearly the ratio of equality. For example; when $n = 110$, $q = 100$, $p = 10$, it is .9938.

22. The ratio of $D b t F$ to $R b t Q$ is less than that of $1 + 2 E \frac{a^p b^q}{n} + \frac{2 E a^p b^q}{n}$ to one. For by Art. 12. the ratio of $D b t F$ to $D b f C$ is less than that of $1 + 2 E \frac{a^p b^q}{n} + \frac{2 E a^p b^q}{n}$ to $1 - 2 E \frac{a^p b^q}{n} - \frac{2 E a^p b^q}{n}$. But by the last Art. $R b t Q$ is greater than the arithmetical mean between $D b t F$ and $D b f C$, and 1 is exactly the arithmetical mean between $1 + 2 E \frac{a^p b^q}{n} + \frac{2 E a^p b^q}{n}$ and $1 - 2 E \frac{a^p b^q}{n} - \frac{2 E a^p b^q}{n}$. From whence this Article is plain.

23. The

23. The ratio of $D b t F$ to $A C F H$ is greater than Σ , and less than $\Sigma \times 1 + 2 E \frac{p}{a} \frac{q}{b^2} + \frac{2 E \frac{p}{a} \frac{q}{b^2}}{n}$. For $D b t F$ being greater than $R b t Q$, the ratio of it to $A C F H$ must be greater than the ratio of $R b t Q$ to $A C F H$, or greater than Σ . Also; since the ratio of $R b t Q$ to $A C F H$ is equal to Σ ; and the ratio of $D b t F$ to $R b t Q$ is less than the ratio of $1 + 2 E \frac{p}{a} \frac{q}{b^2} + \frac{2 E \frac{p}{a} \frac{q}{b^2}}{n}$ to 1; it follows that the ratio compounded of the ratio of $R b t Q$ to $A C F H$, and of $D b t F$ to $R b t Q$, that is, the ratio of $D b t F$ to $A C F H$ must be less than $\Sigma \times 1 + 2 E \frac{p}{a} \frac{q}{b^2} + \frac{2 E \frac{p}{a} \frac{q}{b^2}}{n}$.

24. The ratio of $D b t F + D b f C$ to $A C F H$ (that is, the chance for being right in judging that the probability of an event perfectly unknown, which has happened p and failed q times in $p + q$ or n trials, lies somewhere between $\frac{p}{n} + z$ and $\frac{p}{n} - z$) is greater than $\frac{2 \Sigma}{1 + 2 E \frac{p}{a} \frac{q}{b^2} + 2 E \frac{p}{a} \frac{q}{b^2}}$, and less than 2Σ .

The former part of this Art. has been already proved, Art. 12. The latter part is evident from Art. 21. For $R b t Q$ being greater than the arithmetical mean, between $D b t F$ and $D b f C$, $2 R b t Q$ must be greater than $D b t F + D b f C$; and consequently the

the ratio of $2 R b t Q$ to $A C F H$, greater than the ratio of $D b t F + D b f C$ to $A C F H$ *.

It will be easily seen that this Article improves considerably the rule given in Art. 12. But we may determine within still narrower limits whereabouts the required chance must lie, as will appear from the following Articles.

25. If c and d stand for any two fractions, whenever the fluxion of $c \times F t$ is greater than the fluxion of $d \times C f$ (Vid. fig.) $c \times F t + d \times C f$ will be greater than $Q t$. For in the same manner with Art. 6. it will appear that $c \times F t + d \times C f$ is to $Q t$, as the fluxion of $c \times F t \times 1 + \frac{n z}{q} \times 1 - \frac{n z}{q}$ together with the fluxion of $d \times C f \times 1 - \frac{n z}{p} \times 1 + \frac{n z}{q}$ to the fluxion of $Q t \times 1 - \frac{n^2 z^2}{p q}$. Now since $1 - \frac{n^2 z^2}{p q}$ is the arithmetical mean between $1 + \frac{n z}{p} \times 1 - \frac{n z}{q}$ and $1 - \frac{n z}{p} \times 1 + \frac{n z}{q}$, it is plain, that were the fluxion of $c \times F t$ equal to the fluxion of $d \times C f$, $c \times F t + d \times C f$ would decrease in respect of its own magnitude at the same rate with $Q t$; and, therefore, since at first equal, they would always continue equal. But the fluxion of $c \times F t$ being greater than the fluxion of $d \times C f$ by supposition, and (since q greater than) p $1 + \frac{n z}{p} \times 1 - \frac{n z}{q}$,

* This Art. is true, whether p be greater or less than q .
2 following

also greater than $1 - \frac{n z}{p} \times 1 + \frac{n z}{q}$, it follows that the fluxion of $c \times F t \times 1 + \frac{n z}{p} \times 1 - \frac{n z}{q}$ added to the fluxion of $d \times C f \times 1 - \frac{n z}{p} \times 1 + \frac{n z}{q}$ is greater than these two fluxions multiplied by $1 - \frac{n^2 z^2}{p q}$; and, therefore, greater, than the fluxion of $Q t \times 1 - \frac{n^2 z^2}{p q}$; and, therefore, $c \times F t + d \times C f$ greater than $Q t$.

26. If we suppose three continued arithmetical means between $C f$ and $F t$ ($\frac{3 C f + F t}{4}$, $\frac{C f + F t}{2}$, $\frac{3 F t + C f}{4}$) $Q t$ will be greater than the second, and less than the third, if p is greater than 1. That $Q t$ will be greater than the second has been already proved; and that it will be less than the third, will be an immediate consequence from the last Article, if it can be shewn that the fluxion of $\frac{3 F t}{4}$ is greater than the fluxion of $\frac{C f}{4}$. This will appear in the following manner. The ratio of the fluxion of $C f$ to the fluxion of $F t$ is by Art. 7. and 14.

$$\frac{1 - \frac{n z}{p} \times 1 + \frac{n z}{q}}{1 + \frac{n z}{p} \times 1 - \frac{n z}{q}} \times \frac{q-1}{p-1}.$$

The hyperbolic logarithm

of this ratio is $\frac{1}{p} - \frac{1}{q} \times 2n \approx -\frac{1}{p^2} - \frac{1}{p^3} - \frac{1}{q^2} +$
 $\frac{1}{q^3} \times \frac{2n^3 z^3}{3} - \frac{1}{p^4} - \frac{1}{p^5} - \frac{1}{q^4} + \frac{1}{q^5} \times \frac{2n^5 z^5}{5}$, &c.
 This ratio by Art. 18. is greatest at the point of con-
 trary flexure, or when $z = \frac{\sqrt{pq}}{\sqrt{n^2 - n^2}}$. Substitute this

for z in the series, and it will become $\frac{1}{p} - \frac{1}{q}$
 $\times \frac{2\sqrt{pq}}{\sqrt{n^2 - 1}} - \frac{1}{p^2} - \frac{1}{p^3} - \frac{1}{q^2} + \frac{1}{q^3} \times \frac{2p^{\frac{3}{2}} \times q^{\frac{3}{2}}}{3 \times n^{\frac{3}{2}} - 1^{\frac{3}{2}}}$,
 &c. which, therefore, expresses the logarithm of the
 ratio when greatest, and will easily discover it in
 every case. 'Tis apparent that the value of this
 series is greatest when p is least in respect of q . Sup-
 pose then $p = 2$, and q infinite. In this case, the
 value of the series will be 1.072, and the num-
 ber answering to this logarithm is not greater than
 2.92. The fluxion, therefore, of Cf , when greatest,
 cannot be three times the contemporary fluxion of
 Ft ; from whence it follows that the fluxion of
 $\frac{3Ft}{4}$ must be greater than the fluxion of $\frac{Cf}{4}$.

It is easy to see how these demonstrations are to be
 varied when q is less than p , and how in this case similar
 conclusions may be drawn. Or, the same conclu-
 sions will in this case immediately appear, by changing
 p into q and q into p , which will not make any differ-
 ence in the demonstrations.

In the manner specified in this Article we may al-
 ways find within certain limits how near the value of
 Qt comes to the arithmetical mean between Ft and
 Cf , which limits grow narrower and narrower, as

p and q are taken larger, or their ratio comes nearer to that of equality, 'till at last, when p and q are either very great or equal, $Q t$ coincides with this mean. Thus, if either p or q is not less than 10; that is, in all cases, where it is not practicable without great difficulty to find the required chance exactly by the first rule, $Q t$ will be greater than the fourth, and less than the fifth of seven arithmetical means between $C f$ and $F t$.

27. The arithmetical means mentioned in the last Article may be conceived as ordinates describing areas at the same time with $Q t$; and what has been proved concerning them is true also of the areas described by them compared with $R b t Q$.

28. If either p or q is greater than 1, the true chance that the probability of an unknown event which has happened p times and failed q in $\overline{p+q}$ or n trials, should lie somewhere between $\frac{p}{n} + z$ and $\frac{p}{n} - z$ is less than 2Σ , and greater than $\Sigma +$

$$\frac{\Sigma \times 1 - 2 E a^p b^q - 2 E a^p b^q}{1 + E a^p b^q + \frac{E a^p b^q}{n}}.$$

If either p or q is

greater than 10, this chance is less than 2Σ , and

$$\Sigma \times 1 - 2 E a^p b^q - 2 E a^p b^q$$

greater than $\Sigma +$

$$\frac{\frac{\Sigma}{2} E a^p b^q + \frac{E a^p b^q}{22}}{1 + \frac{1}{2} E a^p b^q + \frac{E a^p b^q}{22}}$$

This is easily proved in the same manner with Art. 12, 23, 24.

That it may appear how far what has been now demonstrated improves the solution of the present problem, let us take the fifth case mentioned in the Appendix to the Essay, and enquire what reason there is for judging that the probability of an event concerning which nothing is known, but that it has happened 100 times and failed 1000 times in 1100 trials,

lies between $\frac{10}{11} + \frac{1}{110}$ and $\frac{10}{11} - \frac{1}{110}$. The second rule as given in Art. 12. informs us, that the chance* for this must lie between .6512, (or the odds of 186 to 100) and .7700, (or the odds of 334 to 100). But from the last Art. it will appear that the required chance in this case must lie between 2Σ , and

$$1 + \Sigma \times \frac{1 - E a^p b^q - 2 E a^p b^q}{1 + \frac{1}{10} E a^p b^q + \frac{E a^p b^q}{10 n}}; \text{ or, between}$$

.6748 and .7057; that is, between the odds of 239 to 100, and 207 to 100.

In all cases when x is small, and also whenever the disparity between p and q is not great 2Σ is almost exactly the true chance required. And I have reason to think, that even in all other cases, 2Σ gives the

* In the Appendix, this chance, as discovered by Mr. Bayes's second rule, is given wrong, in consequence of making m^2 equal to $\frac{n^2}{p q}$, whereas it should have been taken equal to $\frac{n^2}{2 p q}$ as appears from Article 8.

true chance nearer than within the limits now determined. But not to pursue this subject any further; I shall only add that the value of 2Σ may be always calculated very nearly, and without great difficulty; for the approximations to the value of $E a^p b^q$, and of the series $m - z \frac{m^1 z^3}{3} + \frac{n-2}{2 n} \times \frac{m^5 z^5}{5}$, &c. * given in the Essay, are sufficiently accurate in all cases where it is necessary to use them.

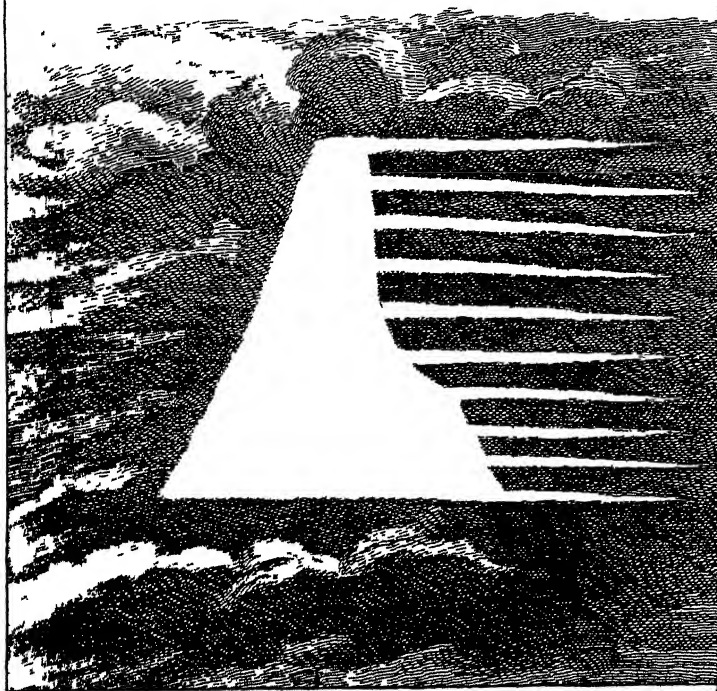
* In the expression for this last approximation there is an error of the press which should be corrected; for the sign before the fourth term should be — and not +.

LIII. *An Account of a remarkable Meteor seen at Oxford, March 5, 1764. In a Letter to the Rev. Thomas Birch, D. D. Secretary to the Royal Society, from the Rev. John Swinton, B. D. F. R. S. Member of the Academy degli Apatisti at Florence, and of the Etruscan Academy of Cortona in Tuscany.*

Good Sir,

Read Dec. 6,
1764.

COMING out of Christ-Church common-room into the great quadrangle, on Monday, March 5th, 1764, about 7^h 30' P. M. I observed, with some surprize, a general brightness in the air, much superior to that of the full moon; though the heavens were then in some measure overcast, and the moon only three days old. This unusual and very remarkable illustration of the atmosphere continued the whole evening, though nothing farther meriting any particular regard (at least nothing that I either saw or heard of) for two or three hours occurred. But throwing up my bedchamber sash, a little before eleven o'clock, I unexpectedly discovered a most glorious and exceedingly resplendent white [TAB. XVIII.] column in the southern part of the hemisphere, which in lustre surpassed every thing of the same kind that I had ever seen before. The base of this column seemed to be between twenty and thirty degrees distant from the horizon, and was many degrees broad. The meteor ascended gradually near thirty degrees, passing to the S. of the zenith.



zenith. It was much narrower at the vertex than the base, and consequently approached somewhat towards a pyramidal form. It remained a few minutes in a fixed and permanent state, after it had arrived at its greatest altitude, and was completely formed. About 11^h 15' P. M. it grew fainter, and much less vivid; and there then darted from it towards the W. several whitish rays and coruscations. At 11^h 20' the lucid column was barely visible, declining apparently southward, and soon after totally disappeared. I went to bed at 11^h 30' P. M. when the atmosphere was covered with the same kind of luminous vapour, that before the formation of the bright Colossean pillar had appeared; and, in the Southern part of the hemisphere, diversified by undulations of shining matter, that exhibited a most beautiful and agreeable scene.

It may not be improper here to remark, that a meteor, called an *Aurora Borealis*, was seen at Lisbon, according to * one of the public papers, the very same night. It is said to have lasted about four hours, and to have engaged the attention of the philosophers there. As from the similitude of certain circumstances it might have been denominated an *Aurora Borealis*, though appearing in the southern part of the heavens, as that I observed actually did; a more particular and distinct account of this phenomenon might possibly enable us to determine, with some degree of probability, whether or no it was the same with that by me here so imperfectly described. Could the identity of these meteors be clearly evinced, or indeed rendered probable, several curious corolla-

* LLOYD'S *Evening Post*.

ries, relative to the altitude, motion, velocity, &c. and even the very nature itself, of that I had so transient a view of, might perhaps be deducible from it.

Instances of *Auroræ Australes*, at least in our part of the world, are immenſely rare. At preſent that obſerved by John Martyn, M. D. F. R. S. and Profeſſor of Botany in the Univerſity of Cambridge, only occurs. The account of this very uncommon appearance, tranſmitted by that ingenious gentleman to the Royal Society, and publiſhed by them in the *Philoſophical Tranſactions* *, highly merits the attention of the curious meteorologiſt. This phænomenon, ſeen by him, January 23, 1749-50, and that of which I now ſend you ſo imperfect a deſcription in ſeveral reſpects pretty well agreed; but in others, which it would be ſuperfluous even to touch upon here, almoſt totally differed. Some of the † public papers informed us, that an extraordinary phænomenon was obſerved in the air at London, the preceding night, viz. March 4, 1764; which in a few particulars reſembled that of which I have taken the liberty to communicate to you a ſhort account in this letter, but in the reſt thoſe two meteors were diſſimilar enough. Perhaps theſe *Auroræ Australes* may ſome time or other hereafter be more frequent than hitherto they have been, and by certain new circumſtances attending them farther elucidate the theory of this ſpecies of meteors; the true cauſe of which, notwithstanding the labours and reſearches of ſeveral

* *Philof. Tranſact.* Vol. XLVI. p. 319.

† *The St. JAMES's Chronicle*, &c. N^o 468.

learned men, seems not yet to be perfectly known.
I am, with all possible consideration and esteem,

S I R,

Your much obliged,

and most obedient servant,

Christ-Church, Oxon.
Aug. 22, 1764.

John Swinton.

LIV. *Extract of a Letter from Mr. John Horsley, Fourth Mate on board the Glatton East-India Ship, to the Rev. Mr. Nevil Maskelyne, F. R. S. dated Batavia, Nov. 16, 1763, giving an Account of his Observations, at Sea, for finding out the Longitude by the Moon.*

Dear Sir ;

Read Dec. 13, 1764. **Y**OU was so good as to express a desire of hearing from me, by every opportunity, during the time of my voyage ; a request that I shall always comply with, with a great deal of pleasure.

I have the misfortune to inform you of our having lost our passage to China, the original occasion of which was our late departure from England. We

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U u

arrived

arrived at Bencoolen July 29, where we were detained till the 13th of September. We found the winds strong set against us in the China seas, which obliged us to bear away for this place.

I shall now give you an account of the observations I have made, since I have been out, and the success attending them.

We sailed from Spithead March the 8th, 1763; the 19th I had four observations of the distance of the Moon from the Sun; by taking the medium the longitude agreed exactly with that by account. The 21st I had another observation, and, the same day, saw the island of Madeira, the body of which, according to this and the former observations (they agreeing exactly) I made to lie $17^{\circ} 18'$ west of London, which differs only 18 minutes from what it is laid down in the chart. The success I met with in this first attempt gave me great satisfaction, and made me continue my observations regularly to the island of St. Paul's, which we made July 5. The day before I had ~~three~~ observations of the distance of the Moon from the Sun. July the 5th, the body of the island bearing by the azimuth compass S. 27° W. distance six leagues, the sky remarkably clear and fine, and the ship having hardly any motion, circumstances all in my favour, I took nine observations of the distance of the Moon from the Sun, the captain and chief mate assisting me in taking the altitudes. I divided them into three sets, and worked from the medium of every three; by which I made the longitude of the ship as follows, $75^{\circ} 15'$, $75^{\circ} 25'$, $74^{\circ} 40'$. The three observations, I took the day before, made the longitude of the ship $74^{\circ} 38'$ and $73^{\circ} 32'$ which brought

brought forward to the noon of July the 5th made $75^{\circ} 45'$ and $74^{\circ} 39'$. Taking the medium of the whole five setts I made the longitude of the ship at noon $75^{\circ} 8' 48''$ east of London. Subtracting from thence the difference of longitude, the bearings and distance of the island gave $= 8' 37''$ west, I made the longitude of St. Paul's $75^{\circ} 0' 11''$ east of London, and $58^{\circ} 0' 11''$ from the Cape of Good Hope. By my account kept from an observation taken June the 18th, I made it $73^{\circ} 35'$ east of London, and $56^{\circ} 35'$ from the Cape, which differs $1^{\circ} 25'$ from what I make the true longitude: most of the accounts on board were between two and three degrees to the westward of mine. The longitude of this island having never been determined by any other method than the runs of ships to the Cape, there are hardly any two charts or books that lay it down alike, they differing from 71° to 74° in their accounts, which made me put little dependance upon any of them.

On our arrival at Bencoolen I took three observations of the distance of the Moon from the Sun, in the road, by which I made Fort Marlborough to lie in $103^{\circ} 50' 45''$ east of London.

I was on shore five or six days in hopes of getting some observations of Jupiter's Satellites, but was disappointed by the cloudiness of the nights; so that I got nothing for my pains but a fever, which had nigh cost me my life, terminating at last in an intermitting one, which has continued with me ever since, neither does it seem to have any inclination to leave me at present.

I have saved all the observations I have made, and the work of them, which I should have sent you a complete copy of, if I had been well enough to have transcribed them.

I am,

Yours, &c.

John Horsley.

“ Mr, Horsley, whose skill and diligence are better evinced by his own account than by any encomiums I can give them, made use of a quadrant made by Mr. Bird, and my British Mariner’s Guide, for determining the longitude of the ship at sea.

N. Maskelyne.

LV. *An Account of a remarkable Meteor seen at Oxford, April 23, 1764. In a Letter to the Rev. Thomas Birch, D. D. Secretary to the Royal Society, from the Rev. John Swinton, B. D. F. R. S. Member of the Academy degli Apatisti at Florence, and of the Etruscan Academy of Cortona in Tuscany.*

Good Sir;

Read Dec. 13,
1764.

HAVING taken a turn on the Parks, or Public University-walk here, on Monday April 23, 1764, towards the decline of the afternoon; I made a visit to a friend in town, with whom



whom I have now and then an article of business to transact. Upon my return home, about 8^h 10' P. M. looking over the houses opposite to Alban-Hall; I observed a very remarkable kind of light, forming the representation of an exceeding bright crepusculum, or expanded body of vapour, which diffused itself over all the northern part of the hemisphere that presented itself to my view. This I looked upon as a prelude to an *Aurora Borealis*, in some form or other. But as such appearances are pretty common here, especially of late years, I then paid no great attention to it. About 8^h 55', not thinking of what I had seen, I threw up my sash, and accidentally cast my eye towards the N. W. where, to my very great surprise, I discovered a luminous arch, [TAB. XIX.] extending itself to the opposite part of the heavens, somewhat resembling an *Iris*, but of a bright white colour. I then went out into the street, traversed part of the town, and found the arch both in the N. W. and S. E. to be nearly terminated by the horizon; so that it seemed to be almost perfectly semicircular, and consequently in a manner to bisect the hemisphere, when completely formed. The meteor was not exactly erect, but ascended obliquely, declining a little to the N. of the zenith. It was extremely narrow, in breadth scarce exceeding two degrees. Its edges towards the S. E. were not so well defined, but somewhat jagged and unequal. From 9^h to 9^h 15' it exhibited a most vivid resplendent whiteness, such as, I believe, was hardly ever observed before. During that term, the phenomenon seemed altogether fixed and permanent, without increase or diminution, without any apparent motion
of

of the whole, and indeed almost without the least external variation. An internal undulating motion of the particles constituting the white luminous matter of the arch was nevertheless discernible, from the first to the last moment of its existence. No stars were visible through the vapour itself, but two or three appeared at a small distance from it. These, however, were much obscured by the interposition of some thin whitish clouds, with which that part of the atmosphere was at this time covered. Not the faintest traces of a proper *Aurora Borealis*, either before the first appearance, during the continuance, or after the extinction of the meteor, were to be seen. Several young people were viewing it, when I went into the street; who seemed, according to custom, not a little alarmed at so unusual a sight. One of them told me, that the arch began to be formed about a quarter before nine. In other parts of the city this wonderful phenomenon was likewise observed, both by townsmen and members of the University, not without some degree of astonishment and surprise. A little past nine o'clock the extremities of the arch grew faint, as did soon after the whole body of the luminous vapour itself. About 9^h 20' the summit, or highest part, of the arch, a few degrees to the N. of the zenith, only remained; which continued gradually decreasing 'till 9^h 27', when the whole totally disappeared.

With regard to the weather, the morning of the 23d was dark and lowring; but the remainder of the day, from 10^h 45' A. M. to sun-set, was bright and clear, though cold out of the sun. The wind 'till six P. M. was northerly, and blew pretty fresh,
but

but then came about to the N. W. From that time to 8^h 15^a P. M. it gradually decreased, and was succeeded by almost a perfect calm, the least breath of air being then scarce perceptible. During the continuance of the meteor, this calm remained; and after the extinction of it, the weather was considerably milder than before. The 24th was a warmer day than any we had had since the month commenced, the sun irradiating us from morning 'till evening with his salutary rays. The whole hemisphere and the horizon this evening were clear and serene, the firmament being but slightly interspersed with thin whitish clouds. That part of it near the horizon was tinged with a most beautiful red colour. The sun, just before he emerged out of our hemisphere, perfectly resembled a globe of fire.

I have not yet been able to meet with an instance of a similar phenomenon in any physiological papers, published before the year 1750. But accounts of two or three meteors somewhat resembling that above described, in our *Philosophical Transactions* *, then occurred. However, that of the 23d of April, 1764, differed from one of these in it's extent, as well as the inconsiderable breadth of the zone forming the arch, and the bisection of the hemisphere. From the others it was sufficiently distinguished by it's most vivid resplendent whiteness, without any short, white, vibrating columns attached to it; especially, as it was, neither preceded, attended, nor followed by any streaming luminous rays, or coruscations. I cannot help therefore considering this as a singular

* *Philosoph. Transact.* Vol. XLVI. p. 345. 346, 347, 648, 649.

fort of phænomenon, never hitherto honoured with an adequate description. If it should appear to the Royal Society in the same light, they will excuse the trouble given on this occasion by,

S I R,

Your much obliged,

and most obedient,

humble servant,

Christ-Church, Oxon.

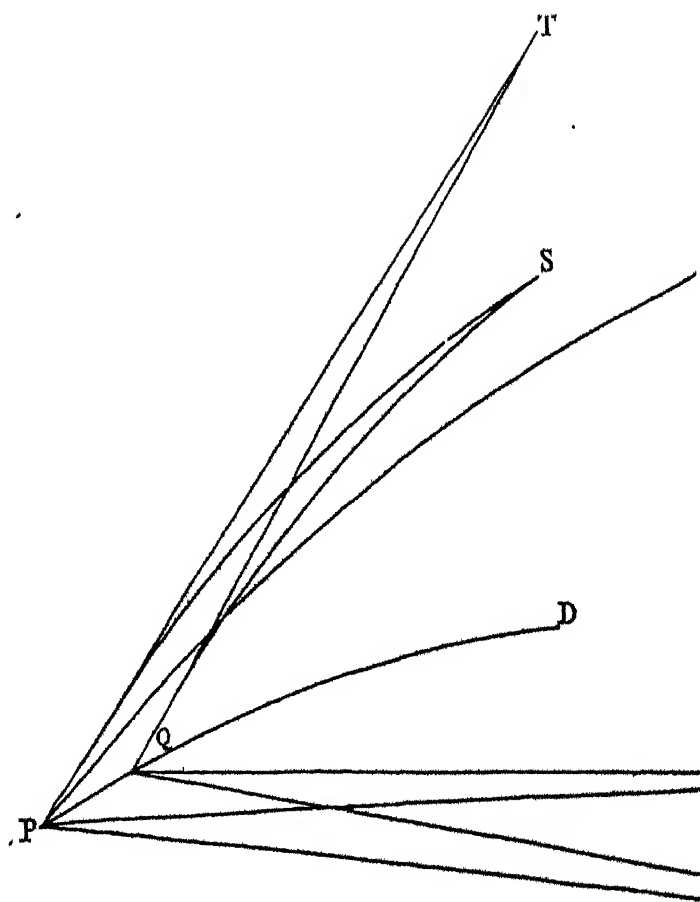
Aug. 29, 1764.

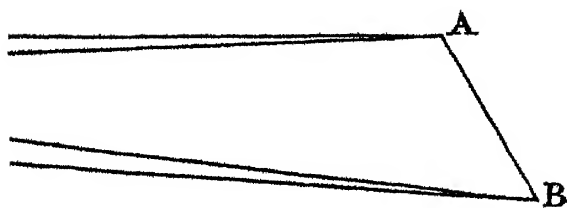
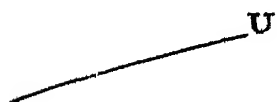
John Swinton.

LVI. *Some Remarks upon the Equation of Time, and the true Manner of computing it.* By Nevil Maskelyne, A. M. Fellow of Trinity College, Cambridge, and F. R. S.

Read Dec. 13, 1764. THESE remarks were wrote above

a twelve-month ago, and would have been then communicated to the Royal Society, had not my voyage to Barbados prevented it. Since my return from thence, I find part of the mistakes here pointed out acknowledged and corrected by M. Delalande, in his Treatise of Astronomy lately published, to whom I remember to have communicated my ideas on the subject, when he was in England. Nevertheless, as the error arising from taking





taking the equation of the equinoctial points into the account still remains uncorrected by him ; and as I flatter myself, that what is said here may tend to set the whole matter in a clear light, I apprehend the publication of these remarks may still be proper.

The French Almanack, called the *Connoissance des Mouvements Celestes*, hath been deservedly esteemed by astronomers, as the most complete publication of its kind. Its present learned editor M. Delalande hath rendered its use more extensive by making the calculations from the latest and most approved tables, and also adding such explanations of them, as, at the same time, lay open before his readers the most considerable improvements of modern astronomy. Nevertheless, as the best mathematicians are not infallible, so I have reason to think I have discovered some errors in M. Delalande's method of computing the equation of time in this ephemeris, or, which comes to the same thing, the mean time, at the instant of apparent noon.

M. Delalande says, page of the *Connoissance* for 1760, which he repeats in the publications of other years, that, "to calculate exactly the difference between
 " mean and true time (that is to say the equation of
 " time) at the instant of apparent noon, the sum of
 " the equation of the sun's centre, the difference
 " between his longitude and right ascension, the lunar
 " equation, the equations of Jupiter and Venus, and
 " that of the precession of the equinoxes, with their
 " proper signs, must be converted into mean solar time.
 " He adds, that it was impossible, before this time,
 " to obtain the equation of time exactly ; 1st, because
 " hitherto no account has been made of the four
 Vol. LIV. X x " little

“ little equations, the sum of which may produce
 “ above three seconds of time; 2dly, because it has
 “ been the practice to convert the equation of the
 “ sun’s centre, and the difference between his right
 “ ascension and longitude into time of the Primum
 “ Mobile, instead of converting them into mean
 “ solar time, which, says he, may produce an error
 “ of two seconds and a half; 3dly, because the
 “ equation of the sun’s centre was not known ex-
 “ actly before, every minute of which answers to
 “ four seconds in the equation of time.”

I readily agree with M. Delalande, that the equa-
 tion of time could not be had so exactly formerly, as
 it may now, when we have a much more exact
 theory of the sun, and are lately made acquainted
 with new equations of his motion. I cannot, how-
 ever, assent to his position, that the equation of the
 equinoctial points is to be taken into this account,
 together with the other equations, since this is not an
 inequality in the sun’s motion, but arises from a mo-
 tion of the equator itself; yet of such a kind as can-
 not accelerate or retard the coming of the sun, or
 any star lying within the tropics, to the meridian, by
 above a quarter of a second of time. This will,
 perhaps, appear in a good measure plain, if it be
 considered, that the diurnal motion of the earth
 round its axis is neither accelerated nor retarded by
 the action of the sun and moon in producing the pre-
 cession of the equinoxes, and variations of the incli-
 nation of the earth’s axis to the ecliptic. The effect
 of these actions is, that the terrestrial pole, each day,
 describes a small arc of a circle about the centre of
 the earth, in the plane of a celestial meridian passing
 through

through the sun or moon, or rather one between both; and, consequently, the equator of the earth has its motion in its own plane neither accelerated nor retarded, but obtains a new motion, whose axis is one of its own diameters. This is the true origin, as well of the minuter and periodical nutations, as of the regular and perpetual motion of the earth's axis about the pole of the ecliptic, observed in all ages, on which the continual precession of the equinoxes depends.

But, to illustrate more fully the point in question, let P, see fig. represent the north pole of the celestial equator, which suppose to be translated, in any certain time, from P to Q, through the small space P Q, upon the meridian P D, by the actions of the sun and moon; let A be the equinoctial point of aries, and S the sun or star. It is evident, that, as the rotation of the earth round its axis is no way affected, the translation of the celestial pole from P to Q along the arch P Q, of the celestial meridian P D, will occasion no alteration in the time of any given meridian of the earth coming to the fixt celestial meridian P D, nor consequently in the time of the sun or stars, when lying in this meridian, appearing to pass the meridian of the given place; contrary to what should follow from the method of computing the equation of time, used in the *Connoissance des Mouvements Celestes*; according to which, as long as the equation of the equinoxes is any thing, the equation of time must be affected thereby, and consequently the absolute time of the sun's passing the meridian.

But, if the sun or star lie not in the celestial meridian $P D$, but in some other meridian $P S$, at S , then the spherical angle SPD is the distance of the sun from the meridian $P D$, when the pole is at P , and $S Q D$ is his distance from the same meridian, when the pole is translated to Q . Let $P T$, $Q T$, meeting in T , be tangents of the meridians $P S$, $Q S$, in P and Q ; $T Q D$ being the external angle of the rectilinear triangle $T P Q$, the angle $P T Q$ is $= T Q D - T P D = S Q D - S P D$, and, therefore, is a measure of the alteration of the time of any meridian of the earth's coming to the sun at S , produced by the translation of the pole from P to Q . Now the sine of $P T Q$ is to the sine of $T P Q$, as $P Q$ to $T Q$; whence, calling the radius unity, and taking $P Q$, on account of its smallness, $=$ the sine of $P Q$, and the angle $P T Q =$ the sine of $P T Q$, we have $P T Q = \frac{P Q \times \text{fine } T P Q}{T Q} =$ the translation of the pole \times the sine of the right ascension of the sun or star reckoned from the meridian in which the pole moves, \div the tangent of the polar distance, or, which is the same thing, \times the tangent of the declination. Therefore, as $P Q$, arising from the nutation of the earth's axis, never exceeds $9'' \frac{1}{2}$, the greatest value of $P T Q$, for the sun can never exceed $9'' \frac{1}{2} \times \text{tangent of } 23^\circ \frac{1}{2}$ the sun's greatest declination, $= 4''$, which answers to about $\frac{1}{4}$ of a second of time: and so much, and no more, may the sun come sooner or later to the meridian, on account of the nutation of the earth's axis: whereas, if the equation of the equinoxes was to be applied directly in the computation, according to

M. Delalande's

M. Delalande's method, it would sometimes, namely when at its maximum of $18''$, produce nearly $1\frac{1}{4}$ second of time.

But, tho' this demonstration may be admitted to be just, yet it may perhaps be asked, wherein lay the fault of the method of computation here censured, and whether the time of the sun's coming to the meridian is not regulated by his right ascension? It may also be thought requisite, that the true manner of computing the equation of time, from the sun's right ascension, should be shewn.

First, let it be observed, that when the pole is at P, A is the equinoctial point, and, when the pole is translated to Q, some other point B is the equinoctial point: therefore the sun's mean right ascension UPA is reckoned from A, and his apparent right ascension BQS , computed from his longitude, corrected by the equation of the equinoxes AB , or BS , is reckoned from another point B. Now the equation of time is proportional to the difference between the sun's mean and true right ascension, both reckoned from the same point; so that if the sun's mean right ascension is reckoned from A, his apparent right ascension, in this case, should be reckoned from A too; or if the apparent right ascension is reckoned, more properly, from the apparent right equinox B, his mean right ascension, for this purpose, should be reckoned from B likewise. For it is plain, from what has been said above, that no small motion of the pole P can at all affect the absolute time of a star in the equator's coming to the meridian of any place; for, the tangent QT then becoming infinite, the angle PTQ vanishes; therefore the mean equi-

nox A will come to the meridian at the same instant of absolute time, as if the pole had not been translated from P to Q; and the difference of time between the sun S coming to the meridian, and a fictitious sun U, supposed to move uniformly in the equator, with a motion equal to the sun's mean motion in longitude, or the equation of time will be therefore measured by $AQS \searrow APU$, the difference of their right ascensions reckoned from the same point A. It will also, by the like reasoning, be measured by $BQS \searrow BPU$, the difference of their right ascensions reckoned from the same point B; for B being the equinox, when the pole is at Q, the absolute time of the point B passing the meridian of any place will remain the same as if the pole had continued at P; whence the proposition easily follows, in like manner as above.

It may be now proper to shew how the equation of time, as affected by the nutation of the earth's axis, ought to be computed. This may be done two ways. The " first follows from what has been just
 " laid down: correct the mean right ascension of
 " the sun U P A, by the precession of the equinoxes
 " in right ascension A P B (which is always to the
 " precession in longitude B A, as cosine of the obli-
 " quity of the ecliptic, to the radius, or as 12 to
 " 13 nearly) the difference of the sun's mean right
 " ascension thus corrected BPU, and the sun's appa-
 " rent ascension BQS, turned into time, is the true
 " equation of time."

Otherwise the effect of the nutation of the earth's axis upon the equation of time, if thought deserving notice, as it can never exceed $\frac{1}{4}$ of a second of time,

time, might be computed from the angle $P T Q = P Q \times \text{fine of } T P D \div T Q$, which, supposing the nutation of the pole to be performed in a circle, whose radius is $8''$, or a mean between the two conjugate semi-axes of the ellipsis, in which it really moves, is $= 8'' \times \text{tangent of the sun's declination} \times \text{cosine of the difference of sun's right ascension, and the longitude of the moon's ascending node.}$

But this is not the only mistake in the computation of the equation of time in the *Connoissance des Mouvements Celestes*, though it may exceed one second of time. M. Delalande says that the sum of the equation of the sun's centre, the difference between his longitude and right ascension, and the sum of the four little equations, must be converted into mean solar time, in order to find the equation of time; and adds, that no exact equation table could be had, before this time, for three reasons, one of which is, that it has always been the practice to convert the equation of the sun's centre and the difference between his longitude and right ascension into time of the *Primum Mobile*, instead of mean solar time, which, says he, may produce an error of $2 \frac{1}{2}$ seconds.

Now I must here freely own, that as I could not, without some reluctance, and only from the fullest proof, allow all the mathematicians and astronomers, before this time, to have been mistaken in the manner of converting the quantities above-mentioned into time, so I can find no reason to conclude so from what has been cited above: on the contrary, from a full consideration of the subject, I apprehend the method hitherto used by the mathematicians to
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be just, and that the author has himself fallen into an equal mistake with that of which he accuses them.

But, in order to set this matter in a clearer light, it will be first necessary to consider motion and time, relatively to each other; for, except this be done, it will be impossible to understand any thing precise from converting a certain number of minutes and seconds into mean solar time, or time of the *Primum Mobile*.

There are three different kinds of time used by astronomers, sidereal time, apparent solar time, and mean solar time. The interval between the transit of the first of Aries across the meridian one day, and its return to it the next day, is called a sidereal day, which is divided into 24 equal parts or hours, and the hours into minutes, &c. This time is shewn by a clock regulated to agree with the transit of the stars across the meridian. The interval between the transit of the sun across the meridian one day, and his transit the next day, is called an apparent solar day, which is divided into hours, minutes, &c. of apparent time. The solar day, it is manifest, and its hours, minutes, &c. are of different lengths, at different times of the year: on account of which inequality, a good clock, which keeps equal time, cannot long agree with the sun's motion, which is unequal. Therefore, astronomers have devised an imaginary time, called mean solar time; which is what would be pointed out by the sun, if his motion in right ascension from day to day was uniform, or, in other words, it is what would be pointed out by a fictitious sun or planet supposed to move uniformly in the equator, with a motion equal to the mean motion

motion of the sun in longitude, its distance from the first point of Aries (meaning hereby the mean equinox) being always equal to the mean longitude of the sun: and as apparent noon is the instant of the true sun's coming to the meridian, so mean noon is the instant at which this fictitious planet would come to the meridian. The interval between its coming to the meridian on any two successive days is a mean solar day, which is divided into hours, minutes, &c. of mean solar time; all which it is manifest will preserve the same length at all times of the year.

The equation of time, at the instant of apparent noon, or of the sun's passing the meridian, being equal to the difference between mean time and 12 hours, is also equal to the interval between the mean and true sun's passing the meridian expressed in mean solar time: to find which, we have the distance of the mean sun from the meridian, at the instant of apparent noon, equal to the difference between the sun's apparent and mean right ascension (both reckoned either from the mean or apparent equinox) which may be called the equation of right ascension. The question, therefore, comes to this, How many minutes and seconds of mean solar time doth the mean sun take to move this distance up to or from the meridian? Astronomers hitherto have allowed 1 minute of time to every 15 minutes of right ascension, and so in proportion; and, I apprehend, justly too; for does not the mean sun, in returning to the meridian, describe 360° about the pole in 24 hours of mean solar time; whence it is plain, that his departure from the meridian is at the rate of 15° to 1 hour, and $15'$ to 1 minute of mean solar time.

Therefore astronomers have not converted the equation of right ascension into time according to the motion of the Primum Mobile; for, the equation of time being mean solar time, and the motion of the Primum Mobile being compleated in 23 H. 56 M. 4 S. of mean solar time, therefore 15° motion of the Primum Mobile does not answer to 1 hour of mean solar time (though it does to 1 hour of sidereal time) but to the 24th part of 23 H. 56 M. 4 S. or 59 M. $50 \frac{1}{2}$ S. And it appears, that the equation of time in the *Connoissance des Mouvements Celestes* has been computed in this manner, and the table in the 79th page of the *Connoissance* for 1761 has been made use of, entitled, "A table to convert into degrees the time of a clock regulated according to the mean motion of the sun." The degrees of this table are evidently degrees of the Primum Mobile, 1 hour of mean solar time giving $15^{\circ} 2' 27,8''$, which answers to the motion of the stars from the meridian, but not to the mean motion of the sun from thence, which is 15° to 1 hour of mean solar time: whence it appears, that this writer hath evidently fell into the mistake of taking motion or space of the Primum Mobile, instead of the mean motion of the sun from the meridian; an equal mistake to that of which he erroneously supposes former mathematicians to have been guilty, in computing the equation of time. So that the equation of time in this ephemeris, besides the mistake arising from the taking in the equation of the equinoctial points into the account, is constantly too small in the proportion of 24 hours to 23 H. 56 M. 4 S. or of 366 to 365, or too small by 1 second upon every 6 minutes of the equation

equation of time : and the mistake of $2 \frac{1}{2}$ seconds, which was supposed to be found in the old manner of reducing the equation of right ascension into time, really takes place in this new method ; which, added to 1 second of time, arising from the mistake in taking the precession of the equinoxes into the account, produces $3 \frac{1}{2}$ seconds, an error which, I apprehend, the astronomical equation tables used since Mr. Flamsteed's time have but rarely exceeded.

To some, who are not well acquainted with the present improved state of astronomy, the difference in question may seem a matter of indifference, and too trifling for notice. But, if truth is the object of all our enquiries, why should we wilfully go beside it in the smallest matters ? And is it not a justice due to past astronomers, to whom we owe the foundations of all our knowledge, to vindicate them even from the smallest censure, which they do not appear to deserve ? At the same time, I flatter myself, that the learned editor of the *Connoissance des Mouvements Celestes*, and also the friends of the late illustrious Abbé de la Caille, who, I believe, was inadvertently the first author of this mistake, will take no offence at my endeavouring to clear up a point, which they, doubtless for want of having given sufficient attention to, seem to have mistaken : since, truth being the common object of all our pursuits, we ought candidly to accept as well the assistance we receive from each other for bringing us into the right road, when we happen to have strayed from it, as for helping us forward on our journey.

The Figure referred to in p. 339. should be TAB. XX.

LVII. *Astronomical Observations made at the Island of St. Helena, by Nevil Maskelyne, M. A. Fellow of Trinity College, Cambridge, and F. R. S.*

T O

THE RIGHT HONOURABLE

The Earl of MORTON, PRESIDENT,

A N D

The FELLOWS of the ROYAL SOCIETY,

T H E

Following Observations made, when I was employed,
by their Appointment, at ST. HELENA,

A R E

Most respectfully presented, by,

His Lordship's,

And the Royal Society's,

Most obedient,

Humble Servant,

Nevil Maskelyne.

T H E

Read Dec. 20, 1764,
and Jan. 20, 1765.

THE following observations were taken with a reflecting telescope, of two feet focal length, made by Mr. Short (of a similar size and construction to those used in the observation of the Transit of Venus, by himself at Saville House, by Mr. Green at Greenwich, and by Mess. Mason and Dixon at the Cape of Good Hope), with an equal altitude instrument made by Mr. Bird, and a clock, with a gridiron pendulum, made by Mr. Shelton, an account of whose going, at Greenwich, before my departure for St. Helena, and immediately upon my arrival there, is contained in Phil. Transf. Vol. LII. Part II. Page 434. and the difference of gravity between those two places thence deduced.

The almost continual cloudiness of the skies, at the Island of St. Helena, renders it a very inconvenient place for the making of astronomical observations, which I had the mortification to experience in losing the sight of the exit of the planet Venus, from the sun's disc, on the 6th of June 1761, to observe which was the primary motive of my going thither. I should have thought myself, in a great measure, compensated for this mischance, if I had been enabled, by the help of the ten foot sector, provided me at the expence of the Royal Society, either to prove or disprove the existence of a sensible annual parallax of the star Sirius, some reasons for the probability of which I laid before the Royal Society, in a paper since published in Phil. Transf. Vol. LI. P. II. p. 889, and, at
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the same time, offered a proposal for the discovery of the same, by observations of the zenith distance of that star, to be made at the island of St. Helena. But, unfortunately, when I came to set up the sector there (which, through the tardiness of the workman in finishing it, I had not had an opportunity of proving, as I had wished, before my departure from England) I soon found a strange irregularity in the observed zenith distances of the stars, amounting to 10, 20, and sometimes even 30 seconds. After having satisfied myself, by various trials, that these great differences in the observations did not arise from any bending of the tube of the telescope, which constitutes the radius of the instrument, or from any looseness in the object-glass, or instability of the wooden three-legged stand, which supports the sector, I, at last, found the cause of error to lie, where I had least suspected it, in the imperfection of the suspension of the plumb-line (which is a fine silver wire) from the neck of the central pin; for, upon taking the loop of the plumb-line off the pin, and putting on again, after turning it half round, or putting on a new one, I found the plumb-line would apply itself to a different part of the limb of the sector, commonly by 10, and frequently by 20 seconds. This experiment, with the same event, I had the honour of exhibiting before a committee of the Royal Society, for their satisfaction, as to the cause of the failure of my intended observations, September 11, 1762, at the British Museum.

The irregularities in question evidently arose from the friction of the loop of the plumb-line against the neck of the central pin; a fault, to which most of
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the sectors, made before mine, have probably been liable. Indeed the fault became more glaring here, by the workman's having made the diameter of the neck of the central pin so large as $\frac{1}{10}$ th of an inch; but that the errors cannot be entirely removed by lessening the neck of the pin, I can assert from my own experience, having caused a pin to be made with the neck only $\frac{1}{20}$ th of an inch in diameter (and beyond that it cannot well be reduced) by which I still found an irregularity in the suspension of the plumb-line, to the amount of 3'', a quantity, though seemingly small, yet of great consequence in the nice observations to which this instrument is generally applied, and which it is capable of taking to a prodigious exactness, when the suspension of the plumb-line is accurately provided for. Mr. Bird has contrived one of six foot length, for settling the limits between Pennsylvania and Maryland, in which the plumb-line is adjusted so as to pass over against, and bisect a small point at the centre of the instrument.

I cannot, on this occasion, omit remarking that the late learned Abbé de la Caille's sector, with which he made his principal observations, from some of which I inferred the probability of an annual parallax of Sirius, seems to have had a like fault with my sector, as may be inferred not only from the differences in the observations themselves, but also from the brief account of the suspension of that instrument, contained in a letter with which I have been favoured by M. Delalande from Paris, an extract of which I presented to the Royal Society. Vide Phil. Trans. Vol. LII. Part 2. Page 607.

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Let me observe also, that the 9 foot sector, made in London by Mr. Graham, with which the gentlemen of the Royal Academy of Sciences at Paris measured a degree of the meridian, at the polar circle, and afterwards a degree between Paris and Amiens, had, at the time of their making those observations with it, the suspension of the plumb-line contrived after the same manner as mine (whatever alterations may have since been made in it) as appears from M. De Maupertuis's minute and accurate description of the said instrument, in his account of the measure of the degree of the meridian between Paris and Amiens: for he there says, that the part of the central pin, on which the loop of the plumb-line was hung, resembled the meeting of two opposite cones at their points; which is an exact description of the form of the neck of the central pin of my sector. But, though this capital defect in the suspension of the plumb-line of my instrument (which I could not correct, at St. Helena, for want of workmen and tools) prevented me from deciding the question concerning the annual parallax of Sirius; yet, as I am conscious, the want of success did not arise from any fault of mine, I shall endeavour to console myself for my disappointment, by the reflexion, that I may, at least, have contributed something to the benefit of astronomy, by having discovered, by my experiments, the imperfection of the above-mentioned method of suspension of the plumb-line in sectors, which no one ever suspected before, and so may be the means of preventing any more instruments of this kind being constructed in the like faulty

faulty manner, and consequently any future astronomers being deceived in their observations.

There still remained one object worthy of attention, which I had also proposed to the Royal Society, and received their encouragement to proceed in it. This was the observation of the horary parallaxes of the moon, by the difference of right ascension in time between the moon's enlightened limb, and stars near her parallel of declination: a kind of observation never before made to my knowledge, by any astronomer, in a latitude so near to the equator, as St. Helena; which, by determining the mean horizontal parallax in that latitude, infers also, by a proportion, which will come out sensibly the same upon any probable hypothesis of the figure of the earth, the mean equatorial parallax, which hath never yet been deduced in any manner so nearly direct.

For the purpose of making these observations, I was provided with a polar axis, suitable to the latitude of the place, on which my reflecting telescope was mounted, and a particular additional eye-piece, having fine silver wires stretched in the focus of the nearest eye-glass. The cell containing the wires being moveable round about, by means of a screw, it was easy to cause any star near the moon's parallel of declination to run exactly along one of the wires, which may be called the directing wire, from the centre to the extremity of the field of the telescope. The exact instants of the stars passing three wires placed perpendicular to the former, which may be called the horary wires, representing small portions of horary circles, were noted by the clock

to the exactness of $\frac{1}{4}$ th of a second of time; as were also the instants of the moon's enlightened limb passing the same wires. It is manifest that the difference of time, observed by the clock, between the star and the moon's limb passing the horary wires, reduced to sidereal time, and from thence into parts of the equator, is the apparent difference of right ascension between the star and the moon's limb passing the horary wires. The same observations repeated after an interval of some hours gave the present difference of right ascension between the star and the moon's limb; whence the moon's apparent motion in right ascension, or the difference of these differences is known; which subtracted from the moon's motion in right ascension, in the given interval of time, owing to her proper motion in her orbit, computed, in the most exact manner, from the best tables, leaves the remainder for the change of the moon's parallax in right ascension between the two times of observation; the ratio of which to the horizontal parallax at that time being also computed, the horizontal parallax of the moon is known: and consequently, by the help of a proportion borrowed from the tables, the mean horizontal parallax of the moon in the latitude where the observations are made. The mean horizontal parallax being deduced in this manner from a great many observations on different nights, the mean of all the results may be taken, as approaching very near to the truth: for the advantage is so great from taking a mean of a great number of astronomical observations, that any degree of exactness required, may

be thereby obtained, provided they are not liable to any constant and uniform cause of error: as has been clearly shewn by my late worthy and learned friend Mr. Thomas Simpson, Phil. Transf. Vol. XLIX. Part I. Page 82; and also in his Miscellaneous Tract, Page 64. Therefore I cannot but think, that, from a considerable number of such observations, the mean horizontal parallax, and thence the mean equatorial parallax of the moon might be deduced certainly to a single second, or ever nearer if required.

In hopes of attaining such a degree of exactness, I endeavoured to multiply my observations as much as possible: yet, through the great cloudiness of the island, could not obtain more than three nights complete observations. If these should appear too few to attain the exactness proposed, yet they may contribute, in a good measure, thereto. However, I have since had an opportunity, during my residence at the island of Barbadoes, in the latitude of 13° north, which 3° degrees nearer the line than St. Helena, to repeat these observations to a very great number, from which, I doubt not, the mean equatorial parallax of the moon may be accurately determined.

I shall here desire to remark, that, if the like observations were repeated in different latitudes, they would probably afford the best means yet proposed for ascertaining the true figure of the earth; as they would determine the ratio of the diameters of the parallels of latitude to each other, the horary parallaxes being proportional thereto: and, after all the experiments and observations that have been made

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on this subject, we shall probably, at last, be indebted to observations of the moon's parallax for the best determination of it: for though the earth affords but a small base at the moon, yet, by repeating these trials, and comparing their results, we may hope to attain that degree of exactness, which we could never expect from fewer observations.

As I look upon the species of observations here spoken of to be of very important use for the improvement of astronomy and geography, and as such desire to recommend the practice of them, especially to those who may have occasion to visit countries of distant latitudes; I shall briefly mention such further particulars, which the experience I have had, and my attention have suggested to me, the observance of which may conduce to the greater accuracy, as well of the observations, as of the consequences to be deduced from them.

I apprehend the use of a polar axis to be very necessary for rightly managing the telescope, as well for finding what star it is proper to compare the moon with, as for preserving the position of the wires unvaried, after their adjustment. A very nice and exact polar axis is not requisite; but a cheap one, and such an one as may easily be provided, will suffice. Mine was formed by a brass socket, making an angle with the horizontal top of the stand equal to 16° or the latitude of the place, receiving the brass cylindrical support of the telescope, instead of the perpendicular socket of the common stand: and the telescope was firmly confined in the socket by a pointed screw which passed through one side of
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the socket into some of the holes, which were drilled in the support of the telescope.

The polar axis may be set near enough to the direction of the meridian, by a magnetic needle, allowing for the variation; or, even by the sight, provided the walls of the observatory be built nearly north and south. This being done, and the directing wire being brought into such a position, that the star may run exactly along it from the centre to the extremity of the field of the telescope; then if the screws of the rack work be turned, and the star be brought back to the intersection of the wires, it will be found to run exactly along the directing wire again; and this I generally found would be the case, even for a very considerable space of time, though the star had, in the mean time, advanced a considerable way from east to west by the diurnal rotation; so that it is not always necessary to re-adjust the wires after each set of observations, though it may be proper to examine whether they require it or not. Hence it follows, that there can be no danger of disturbing the position of the wires after their adjustment, by bringing the star back to the entrance of the telescope, in order to observe its passage across all the horary wires.

Sometimes it so happens that a proper star cannot be found that precedes the moon, to compare her with; in such a case, the observer must compare her with a star following her, and adjust the wires by making some bright point of the moon run along the directing wire, which is a more exact method than by making the directing wire a tangent to the moon's
north

north or south limb. Here, indeed, the directing wire cannot represent a parallel to the equator, on account of the moon's continual change of declination, but will make a small angle therewith; which may be computed, and the observations corrected accordingly. But the correction may be easier made, as follows, let a express the moon's apparent angular motion about the pole of the world in four minutes of time, being the difference of her proper motion in right ascension, and the change of her parallax in right ascension: d her apparent motion and declination in the same time, b the difference of the apparent declination of the moon, and that of the star, r the radius, and c the cosine of the moon or star's declination; the correction to be applied to the moon's right ascension, or the difference of right ascension of the moon and star is $b \times \frac{d}{a} \times \frac{r^2}{c^2}$. If the moon is approaching the star's parallel of declination, she will come to the horary wire relatively too late for the star, and her right ascension, deduced immediately from that of the star, will be too great, and must be diminished by the correction here mentioned; but, if the moon is receding from the star's parallel of declination, she will come to the horary wire relatively too soon for the star, and her right ascension, immediately deduced from that of the star, will be too small, and must be increased by the above-mentioned correction.

There is another attention, which the nice observer will not think too trifling for his notice, namely, to examine whether the wires of his telescope
are

are placed at exact right angles to each other (which they seldom are) and, if they are not, what the quantity of deviation is, in order to make an allowance for it in the reduction of the observations. This may be tried several ways. I examined the wires of my telescope at Barbados, by a great many observations of the difference of right ascension of stars, which differed considerably from each other in declination, namely Arcturus, and the little star accompanying it, and the virgin's spike, and a small star preceding it; first with the wires in the common position, and next when turned a quarter round, making the middle horary wire serve as a directing wire; for, if the wires do not cut each other at right angles, the difference of right ascension of the stars will come out too much one way, and as much too little the other way, and half the difference will be the correction in this case, whence it may be inferred in all other cases. Or, the angle of the deviation of the wires from a right angle being hence found, the correction of the difference of right ascension of the moon and star, is to the difference of their apparent declination; as the sine of the angle of the deviation of the wires is, to the cosine of the moon or star's declination.

I have determined the deviation of the wires of the telescope, which I used at St. Helena, by comparing them with a right angle, formed by two silver wires on a brass plate, fixed up in a window at the distance of 30 feet from the telescope. The extent of the compasses, with which the intersecting arches were struck, for finding the perpendicular
lines

lines on the plate, being no less than seven inches, those wires may be supposed to differ insensibly from a right angle to each other. The telescope being adjusted for seeing them distinctly, I brought that wire of the telescope, which in celestial observations represented a parallel of declination, to be exactly parallel to one of the wires on the plate, with the smallest interval possible; and, at the same time, made the middle perpendicular, or horary wire, to pass through the intersection of both the wires in the window: when I plainly discerned, that the wires of the telescope were not exactly perpendicular to each other, the superior angle to the right being manifestly acute, and the superior one to the left obtuse. This I further verified by applying the acute angle to the left hand superior angle of the plate, turning the wires in the telescope a quarter round, from right to left, by the screw adapted for this purpose, when the same difference appeared as before. This proved also that the wires on the plate made exact right angles with each other; otherwise the acute angle of the wires of the telescope could not have appeared to differ equally from both of them. To find the exact difference of the angle made by the wires from a right angle, I had a third wire placed exactly parallel to one of the former on the plate at the distance of $\frac{1}{8}$ th of an inch; when by applying the angle of the wires of the telescope to the right angle on the plate, the deviation of the former from the latter appeared to be equal to half the interval of the parallel wires at the extremity of the field of view; but the semi-diameter of the field of the telescope at the distance of the wires in the window
being

being measured $\frac{925}{1000}$ th of an inch; whence the angle of deviation of the wires, from a right angle, is 21 minutes. But, by a mean of 11 trials, the quantity of the deviation came out $28' \frac{1}{2}$, the extreme results being $21'$ and $36'$. This is the deviation of the south part of the middle horary wire, from a perpendicularity, to the directing wire towards the east, in the observations at St. Helena; a star, that passed south of the centre of the telescope, coming to the horary wire too soon, and a star that passed north of the centre coming later to the horary wire than it ought to do.

In order to determine whether the two other horary wires were parallel to the middle one, or, if not, what angle they made with it, I compared the transit of 13 stars across the three horary wires, with those of as many more stars differing considerably in declination from the former, all observed at St. Helena; and from the differences of right ascension at the several wires, after making an allowance for the convergence of the meridians, which however is not $2'$, I found the south part of the first or eastern wire to deviate from a parallelism with the middle one towards the west by $9', 8$, and the western wire to deviate towards the east by $5', 4$; hence it appears that the south parts of the eastern, middle, and western horary wire differed from a perpendicularity to the directing wire towards the east, in the observation of St. Helena, by $18', 7$, $28' \frac{1}{2}$, and $34'$, the mean deviation of all the three being $27'$ or only $1 \frac{1}{2}$ different from that of the middle horary wire. This quantity of the mean deviation of the wires is also confirmed

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firmed by a comparison of six differences of right ascension of stars observed at St. Helena, with the same observed, at my desire, since my return, at the Royal Observatory, by the transit instrument, which gives $27' \frac{1}{4}$, agreeing exactly with what has been here found in a more certain manner.

Sometimes, in making these observations, it so happened, that several stars lay near the moon's parallel of declination; when I observed all of them, that came within the field of the telescope; as well to obviate the hazard of missing to observe the right star again after an interval of several hours, as to obtain a greater number of comparisons of the moon's motions in right ascension, and so reduce the unavoidable errors of the observations as much as possible.

It may be proper to remark, that the most convenient time for making these observations, is when the moon is stationary at her greatest declinations; when she may be compared with the same star, with a telescope having a moderate field of view, for several hours. The change of the moon's parallax in declination is then alone to be feared; but if the observations are made nearly at the same distance from the meridian, both on the western and eastern side, the parallax returning to the same quantity, will occasion no difficulty. Sometimes, when the moon is not exactly at, but only near, her greatest declination, by observing her on the proper side of the meridian, the effect of parallax may be found to be contrary to, and consequently counteract, her change of declination arising from her proper motion.

Through

Through the great cloudiness of the skies at St. Helena, I could observe the moon only one night, namely January 8, 1762, near her limits of declination; on the other nights, I endeavoured to compare her with as many stars as came within the field of the telescope, trusting to determine afterwards the difference of right ascension of the stars, with which she was compared in the former and latter observations of the same night. This design I have since completed, by procuring a great many transits of these stars, to be observed on the meridian, at the Royal Observatory, in the latter end of the year 1762, and beginning of the year 1763; many also I made there myself. An account of them is given at the end of the other observations.

An useful remark here offers itself to our notice; that the moon's parallax may be very well determined, in a fixed observatory, at any period of her declination, by observing the difference of right ascension of her limb, and any star near her parallel, at a considerable distance from the meridian, either to the east or west, with the parallactic telescope, and also on the meridian with the transit instrument.

But, in pursuing this method, the parallactic telescope ought to be nearly of equal goodness with the transit telescope; else the moon's diameter might appear greater by some seconds through one and the other, and consequently the parallax so deduced would not be exact. It is true, that, by a proper method of comparing the observations, this small error might be obviated, though the telescopes differed ever so much in the degree of distinctness, namely, by taking a mean of the results found by

the observations in the first and last half of the moon, or on the eastern and western sides of the meridian; for it is manifest the errors would be of contrary tendency in these different cases.

As it may serve the more to recommend the practice of these observations to astronomers, I think it proper to mention, that I seldom failed of finding some proper star or stars near the moon, of sufficient brightness, to compare her with, even when there were none such marked down in any catalogue, or any charts: the number of zodiacal stars proper for comparing the moon and planets with in a telescope, and not inserted in any printed catalogue, seeming much to exceed the number of those marked down.

I have but one more remark to add on this subject, that, as it is necessary to know nearly the apparent difference of declination of the moon's centre, and the stars observed, in order to correct the observations for the deviation of the wires, and the moon's change of declination, so this may most readily and conveniently be done by some oblique wires fixed in the focus of the eye-glass of the telescope, as I had in that I used at Barbadoes. Not having such a contrivance adapted to the telescope I used at St. Helena, I always estimated by my eye how many minutes each star, and also the moon's centre, passed north or south of the directing wire, which I had an easy method of doing, by comparing their distance from the directing wire, with the interval between the directing wire, and one of the two wires stretched parallel to it, at the exact distance of 10 minutes on each side. In this manner the numbers
set

Fig. 1.

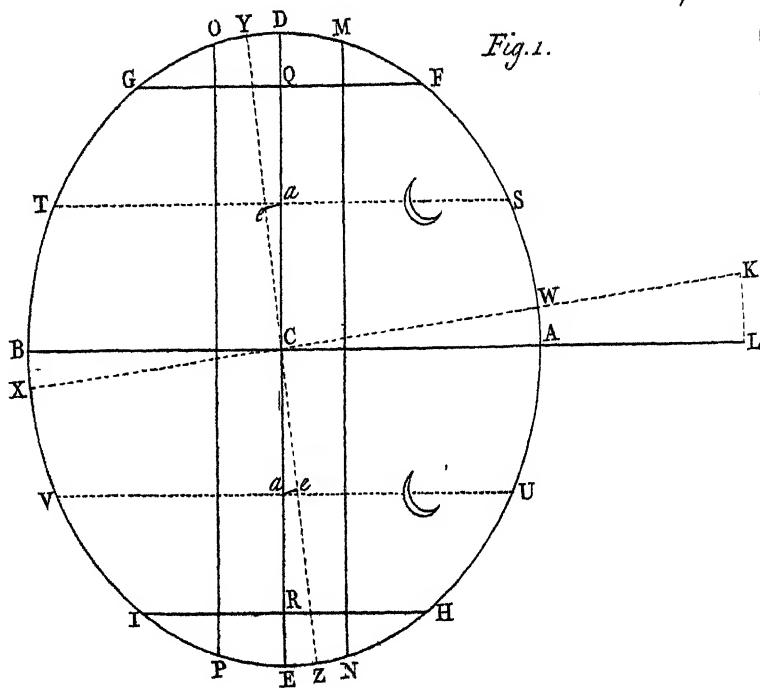
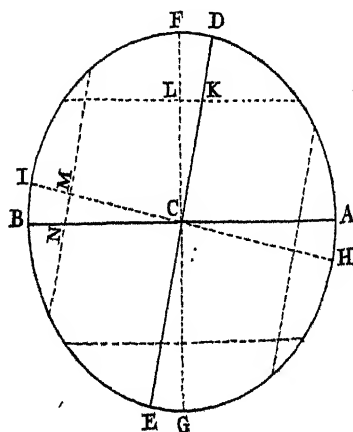


Fig. 2.



set down by the sides of most of the observations were deduced, expressing the number of minutes which the stars passed north or south of the moon's centre. They may be depended on to a minute, or two at most, which is sufficient for the reduction of the observations.

To make the foregoing account of the method of observing the moon's horary parallaxes more clear, let *ADBE*, *TAB.* *XXI.* *fig. 1.* represent the cell containing the silver wires stretched in the focus of the nearest eye-glass of the telescope, of which *AB* is the directing wire, *MN*, *DE*, *OP*, three wires perpendicular to *AB* representing portions of horary circles; and, *FG*, *HI*, two other wires parallel to the directing wire *AB*, at the equal distances, *CQ*, *CR*, on each side, equal to 10 minutes. The semi-diameter of the field of the telescope *CA* is 14 minutes; *A* is the eastern side of the field of view, *B* the western, *D* the southern, and *E* the northern side. In order to adjust the wire *AB* parallel to the equator, so that *MN*, *DE*, *OP*, may represent horary circles, the star, whose difference of right ascension from the moon is to be observed, is brought by means of the screws to the intersection of the wires *AB*, *DE*, at the centre of the telescope *C*, and when the star is near passing out of the telescope at *B*, the cell *ADBE* is turned round, by means of a screw, till the star is again brought upon, and bisected by the wire *AB*; this being done, if the screw be turned to make the telescope follow the star towards the west, and the star be again brought to the intersection of the wires at *C*, it will be generally found to run exactly along the wire *CB*, bisected by it all the way from *C* to *B*; which is then a proof that the wire *AB* is rightly adjusted; but, if the star
runs

runs not exactly along the wire AB , the position of that wire must be altered a little, till the star runs exactly along it from the centre C , to the extremity of the field of the telescope at B . Then turn the screw to carry back the star from B a little to the east of the first or eastern wire MN , and observe the exact minute, second, and quarter of a second, of the stars passing the three horary wires MN , DE , OP ; the telescope remaining unmoved and undisturbed, observe, in like manner, the transit of the moon's enlightened limb across the same wires, whether she pass south of the star, as along the line ST , or north of the same, as along to the line UV ; and the observation is completed. The like observation being repeated, after an interval of several hours, we shall have the apparent motion of the moon in right ascension in this time; whence the moon's horary, and thence her horizontal parallax may be computed.

If the moon precedes the star, and the wire AB is adjusted by making some bright point in the moon run along it, and WX is supposed to be the true parallel of declination, it is manifest that the star will pass the horary wire at a , to the south of the centre of the telescope sooner, and to the north of the centre of the telescope later, than it passes the true horary circle YZ at e , by the time it takes to describe ae parallel to WX . Let CL be the apparent motion of the bright point of the moon, in four minutes of time; draw KL perpendicular to WX produced, and CK is the apparent motion reduced to a parallel of the equator in four minutes, and KL the apparent motion in declination in the same time; and,

the right-angled triangles aeC , KCL being similar, ae is to eC , as KL to CK ; but the error of the right ascension answering to ae is to ae , as radius to the cosine of the star's declination; and CK , is to the moon's apparent angular motion about the pole, in four minutes, as cosine of moon's apparent declination, to radius. Whence, by composition of ratio's, and by equality, the correction of the moon's right ascension is to eC the apparent difference of declination of the moon and star, in a compound ratio of the moon's apparent motion, in declination in four minutes, to her apparent motion about the pole in the same time, and of the square of the radius, to the product of the cosines of the stars and moon's apparent declinations.

Further it appears from the scheme, that the moon comes later to C than to the horary circle passing through the point of the wire (a) cut by a star between C and D , whose parallel of declination she is approaching, and that she comes sooner to C than to the horary circle passing through the point of the wire (a) cut by a star between C and E , whose parallel of declination she is receding from, by the time the star takes to describe ae ; and, therefore, the right ascension of the moon deduced immediately from that of the star must be too great in the first case, and too little in the second case, by the space ae measured upon the star's parallel of declination.

Lastly, to explain the manner of examining the deviation of the wires, from a perpendicular to each other, by observations of the stars; let AB , fig. 2. represent the directing, and ED the middle horary wire, deviating from CF , supposed perpendicular
to

to AB , by the small angle DCF . Let any star be made to run along the wire AB , from A to B , any other star following it will pass the wire DE , at K sooner, than the horary circle FG at L , by the time of its describing the small space LK , and consequently the difference of right ascension will appear too little; now let the wires be turned a quarter round, that the wires AB , DE , may change places, D coming into the place of A , and E into that of B , which is done by making the first star run along the wire DE , from C to E . Now the wire AB , deviating from CI perpendicular to DE , by the angle BCI , the second star will pass the wire BC at N , later than the horary circle IC at M , by the time it takes to describe the space $MN = LK$; and consequently the difference of right ascension of the two stars will appear as much too great, as it before appeared too little, when the wires were adjusted in their usual position: and half the difference will be the correction in this case, to be added to the first, or subtracted from the second difference of right ascension; whence the correction may be easily inferred for all other observations.

CELESTIAL OBSERVATIONS.

Eclipse of the Moon, May 18, 1761.

Appt Time.

H ,

7 48 38	Penumbra plainly entered upon the Moon's disc.
7 56 23	Beginning of the Eclipse.
7 19 16	Shadow begins to touch Kepler.
7 37 8	Shadow bisects Manilius.
7 40 8	Shadow begins to touch Julius Cæsar.
10 39 23	Emerfion out of total darknefs.
11 46 52	End of the Eclipse.

Immersion and Emerfions of Jupiter's Satellites, observed at the Observatory on the Alarum Hill.

1761 Day of the Month.	Appt Time			
	H	,	..	
7 July 20	17	44	31	Imm. 1 Sat. The Moon near Jupiter.
8 July 22	12	12	29	Imm. 1 Sat.
8 Aug. 5	14	49	50	3 Sat. almost immerg'd. Then clouds.
	14	50	30	3 Sat. certainly immerg'd.
	16	0	40	Imm. 1 Sat.
24 Aug. 27	11	13	56	Emerf. 4 Sat.

Immersion and Emergence of Jupiter's Satellites, observed
in James's Valley.

1761.		Apparent Time.		
Day of the Month.		H	M	
h	Oct. 10	12	48 38	Em. 2 Sat.
24	Oct. 15	13	36 10 :	Em. 1 Sat.
2	Oct. 16 {	10	3 40	Em. 3 Sat.
		10	3 45	{ Ditto by Mr. Mafon (who arrived here to-day from the Cape of Good Hope) with a two foot reflecting telescope.
D	Nov. 9	8	19 54	{ Em. 1 Sat. Instantaneous both to myself and Mr. Mafon, exactly at the same second, in different houses. Air very clear, and Satellite increased its light very fast.
2	Nov. 11	12	40 19	{ Em. 2 Sat. by Mr. Mafon. I missed the instant of Emergence by moving the stand of my telescope. Air very clear.
D	Nov. 23	12	9 52	Em. 1 Sat.
		7	39 18	Imm. 3 Sat. Air a little hazy.
h	Nov. 28 {	7	39 13	Ditto by Mr. Mafon.
		10	8 17	3 Sat. had not em. Then clouds.
		10	10 13	{ Ditto plainly out, though not near arriv'd to its full lustre.
○	Nov. 29	7	9 48	{ 2 Sat. had emerg'd. Its light was so weak, it probably had not been out above 15 seconds.
○	Dec. 6 {	9	43 36	Em. 2 Sat.
		9	43 33	Ditto by Mr. Mafon.
1762.				
○	Jan. 10	7	37 42	Imm. 3 Sat.

N. B. This mark (:) affixed to any observation signifies that it is a little uncertain.

Observations

Observations of the difference of right ascension, by time, between the moon's enlightened limb and stars, taken by the help of the parallactic wires adapted to the reflecting telescope; designed for determining the horary parallaxes of the moon. They may also serve to deduce the longitude of the place.

24 Sept. 3. Compared the moon's western and preceding limb, with α Libræ in right ascension, at the Observatory on the Alatum Hill. The times are by the clock, which is 12 seconds too slow for sidereal time, and keep the rate of going of sidereal time, exactly. The star passed south of the moon's centre.

	1st hor. w.		2d hor. w.		3d hor. w.		D's Limb at middle wire.		
	H	"	"	"	"	"	H	"	"
α Libræ	17	42.	9	42	23	42	37		
D's W. Limb	17	43	45	43	59 $\frac{1}{2}$	44	13 $\frac{1}{2}$	6	52 49
α Libræ	18	2	10	2	24	2	38		
D's W. Limb	18	4	14	4	28 $\frac{1}{2}$	4	42 $\frac{1}{2}$	7	13 15
α Libræ	18	16	3	16	17	16	31		
D's W. Limb	18	18	27 $\frac{1}{2}$	18	42	18	56	7	27 26
α Libræ	18	23	6	23	20	23	34		
D's W. Limb	18	25	41-	25	55	26	9+	7	34 38

Thursday September, 24 I removed the clock down to James's Valley, and keeping the same length of the pendulum as before, fixed it up strongly against the wall of a house, in an upper room, whence I could make my observations through openings made in the roof of the house. I fixed the equal altitude instrument, for regulating the clock, against a strong post, let deep into the ground, in a little room eight foot square, built for this purpose, in a convenient open place, at a little distance from

the house where the clock was. When I observed the sun's equal altitudes, I first adjusted the instrument; then I went to the room where the clock was, and set my watch, having a second hand, exactly with it; then I returned to the equal altitude instrument, and observed the passage of the sun's limb across the horizontal wires of the instrument, according to the time shewn by the watch; and, immediately after the observation, went again to the clock, and compared the watch with it, noting how much it had got or lost, whence the observations were easily reduced to the time of the clock.

		1 Wire	2 Wire	3 Wire	Apparent Time					
		H	'	"	'	"	H	'	"	
24 Oct.	8	♄'s W. L.	11	34	2	34	17	34	31½	10 45 14
		♄ Capricorni	11	Clouds	36	30½	Clouds	Star	10' N. of M.	
		H	'	"	'	"	H	'	"	
10 Oct.	10	♄'s W. L.	14	19	57	20	12-	20	25	13 23 7
		♄ Aquarii	14	24	57	25	11	25	24½	

October 8th and 10th the clock got 7", 3 upon 24 hours in one revolution of the fixed stars.

The foregoing observations were all made with the telescope placed upon the common stand, without the polar axis. The observations of December 4th, that follow, were made with the telescope fixed upon a new and heavier stand, which was rendered more steady, by two broad feet resting upon several of the boards at once. The socket for receiving the telescope was cut obliquely in the stand, so that it had partly the effect of a polar axis.

October 28th, I took down the clock, packed it up, and sent it on board a vessel going to the Cape of Good Hope, to return again soon, committing it to the care of Mr. Jeremiah Dixon, who had observed the transit of Venus over the sun at the Cape. He took his passage on board the said vessel, in order to set the clock up at the Cape of Good Hope, and examine the difference of its going between that place and St. Helena, for determining the proportion of the force of gravity at those two places.

The same day Mr. Maſon fixed his clock up, for my use, against a large maffy poſt, let deep into the ground, near the equal altitude instrument, at the little Obſervatory. This clock was made by Mr. John Ellicott, F. R. S.

I still continued, for some time, to make my observations in the upper room, as before. For this purpose I fixed up a little clock there, which may be called a journeyman, or secondary clock, having a pendulum swinging seconds, which after being well adjusted, would keep time very regularly for several hours. It had only a minute and second hands, and struck every minute exactly as the second hand came to sixty, which, was very convenient for the counting of seconds; more especially in the observations made with the parallactic telescope, it being improper, on account of the instability of the floor, to get up from one's seat, or to alter the position of the body considerably even to catch the second, till those observations were completed. I reduced the times to that of the observatory clock, by means of my watch, with the second hand. The little clock, as well as the larger clock, which I sent with Mr. Dixon to the Cape of Good Hope, was made by Mr. John Shelton.

‡ December 4th, I compared the moon's western limb with the three γ of Aquarius, with respect to right ascension, and observed her occult to the southern one. The time is set down according to the little clock, and the difference between that and the observatory clock is set down by the side; the latter lost 59 seconds upon 24 hours, in one revolution of the fixed stars, and the little clock kept very nearly the rate of sidereal time. In these observations the directing wire was adjusted by the stars.

	1 Wire	2 Wire	3 Wire	Lit. clock. flow. than obs. clock	D's L. at mid. Wire by observa- tory clock.	Appt Time
	H , "	, "	, "	, "	H , "	H , "
D's W. Limb 3d * Aquarii	23 21 0 23 23 32	21 13 ¹ / ₂ 23 46—	21 27 23 59—	8. 4	23 29 17 ¹ / ₂	7 19 5
D's W. Limb 3d * Aquar.	23 38 31 23 40 46	38 45 40 59 ¹ / ₂	38 59 41 13	8. 4	23 46 49	7 36 34
D's W. Limb 3d * Aquar.	0 47 17 0 48 18	47 31— 46 32	47 44 48 45 ¹ / ₂	8. 0	0 55 31	8 45 6
D's W. Limb 3d * Aquar.	0 58 0 0 53 50	58 15— 59 3 ¹ / ₂	58 28 59 17	8. 0	1. 6. 15	8 55 48
2d * Aquar. D's W. Limb	1 4 10 1 4 30	4 24 Clouds	4 37 4 58+	8. 0	1 12 44	9 2 17
D's W. Limb 3d * Aquar.	1 17 33 Clouds	17 47 18 13 ¹ / ₂	18 1 Clouds	7 59	1 25 46	9 15 17
2d * Aquar. D's W. Limb	1 25 7 ¹ / ₂ 1 25 54	25 21 ¹ / ₂ 26 8	25 35 26 22	7 59	1 34 7	9 23 37
1st * Aquar. 2d * Aquar. D's W. Limb	1 31 27 1 33 33 1 34 30	31 40 33 47 34 44	31 54 34 0 34 58	7 58	1 42 42	9 32 10
1st * Aquar. 2d * Aquar. D's W. Limb	1 45 31 ¹ / ₂ 1 47 37 1 48 53 ¹ / ₂	45 45 ¹ / ₂ 47 52 49 7 ¹ / ₂	45 59 ¹ / ₂ 48 5+ 49 21	7 58	1 57 5	9 46 31
1st * Aquar. D's W. Limb	1 58 57 2 2 37 ¹ / ₂	59 11 2 52	59 24 ¹ / ₂ 3 5 ¹ / ₂	7 57	2 10 49	10 0 13
1st * Aquar. D's W. Limb	2 9 16 2 13 11	9 30 13 25	9 43 13 39	7 57	2 21 22	11 10 45
1st * Aquar. 2d * Aquar. D's W. Limb	2 20 6+ Clouds 2 24 17	20 20 22 26 24 31 ¹ / ₂	20 33 ¹ / ₂ Clouds 24 45	7 56	2 32 27	10 21 48

At 23 H. 49 M. 48 S. by little clock, or 23 H. 57 M. 51 S. by observatory clock, which is 7 H. 47 M. 34 S. apparent time, the 3d γ of Aquarius vanished instantaneously, clouds coming over the moon at the same time. Therefore it remains a little dubious, whether this was the very instant of the star's occultation by the moon, or whether it was obscured by the clouds, though I rather suppose the former from the manner of its vanishing, and also because when the clouds cleared away presently the star was gone.

December 4th, by equal altitudes, the sun passed the meridian at 16 H. 9 M. 11,3 S. and December 5th at 16 H. 12 M. 33,4 S. by the observatory clock, whence the observed times are easily reduced to apparent time, as above.

Finding the above observations of December 4th (though they may be depended on to half a second of time) to be still incommoded by a small trembling of the telescope, owing to its resting on a floor; I determined, for the future, to make these observations, at the little observatory, on the ground, which I caused to be altered, to make it more convenient for this purpose. Here I constantly made use of the polar axis, which I found to afford considerable advantages with respect to the facility and exactness of making the observations.

♀ January 8th 1762. Compared the moon's western limb, with several stars, with respect to right ascension, at the little observatory. The four stars, with which the moon was compared, are distinguished by letters, according to the order of their right ascension.

	1st Wire	2d Wire	3d Wire	Stars North or South of D's centre	D's Limb at Mid. Wire
	H " "	" " "	" " "	" "	Apparent Time H " "
<i>a</i>	2 48 30—	48 45	49 0	13 $\frac{1}{2}$ N.	
<i>d</i>	Clouds	55 53+	55 48+	4 $\frac{1}{2}$ N.	
<i>e</i>	5 56 10+	56 25+	Clouds	15 $\frac{1}{2}$ N.	
	2 57 3	57 18	Clouds		8 41 58
<i>a</i>	3 16 32	17	17 22+	14 $\frac{1}{2}$ N.	
<i>b</i>	3 19 12—	19 27	19 42+	22 N.	
<i>c</i>	3 22 38	22 53+	Clouds	13 $\frac{1}{2}$ N.	
<i>d</i>	3 26 16	26 32	26 47 $\frac{1}{2}$		9 11 8
<i>a</i>	4 22 38—	22 53—	23 8—	12 N.	
<i>b</i>	4 24 57	25 13—	25 27 $\frac{1}{2}$	19 N.	
<i>c</i>	4 Clouds	28 39	28 54	11 $\frac{1}{2}$ N.	
<i>e</i>	4 30 18	Clouds	30 48—	12 $\frac{1}{2}$ N.	
<i>d</i>	4 33 55	34 11 $\frac{1}{2}$	34 27		10 18 38
<i>a</i>	4 52 44—	52 59	53 14		
<i>b</i>	4 Clouds	55 19	55 34	<i>b</i> 7 N. of <i>a</i>	
<i>d</i>	5 4 54 :	5 9 $\frac{1}{2}$:	Clouds		10 49 31 $\frac{1}{2}$
	5 15 9	Clouds	15 39 $\frac{1}{2}$		
	5 20 4 $\frac{1}{2}$	20 20	20 35 $\frac{1}{2}$		11 4 40
*	5 30 52	31 7 $\frac{1}{2}$	31 22 $\frac{1}{2}$		
<i>a</i>	5 33 15	33 30	33 45	20 circ. of S. *	
<i>a</i>	5 57 39	57 54+	58 9+	6 $\frac{1}{2}$ N.	
<i>b</i>	5 59 59+	0 14 $\frac{1}{2}$		13 $\frac{1}{2}$ N.	
<i>c</i>	6 3 25 $\frac{1}{2}$	3 40 $\frac{1}{2}$	3 55 $\frac{1}{2}$	6 $\frac{1}{2}$ N.	
<i>e</i>	6 5 20	5 35	5 50	7 $\frac{1}{2}$ N.	
<i>d</i>	6 11 44	12 0—	12 15		11 56 12
*	6 18 18 $\frac{1}{2}$	18 34	18 49	20 cir. S. of *	
<i>a</i>	6 20 43	20 58	21 13	13 $\frac{1}{2}$ S. of ditto	
<i>b</i>	6 23 2	Clouds	23 32 $\frac{1}{2}$	21 $\frac{1}{2}$ S. of ditto	
<i>c</i>	6 26 28+	26 43 $\frac{1}{2}$	26 59	19 $\frac{1}{2}$ S. of ditto	
<i>e</i>	6 28 23	28 38+	28 53—		
	7 55 56	56 11 $\frac{1}{2}$	56 26+	10 S.	
	8 5 56	6 12	6 27		13 50 9 h January

January 9. Compared \mathcal{D} 's western limb with three stars.
Adjusted the directing wire by a bright point in the moon.

	1st Wire	2d Wire	3d Wire	Stars N. or S. of \mathcal{D} 's centre	\mathcal{D} 's Limb at Mid. Wire
	H ' "	' "	' "	' "	Apparent Time H ' "
\mathcal{D} L.	3 59 42	59 58	0 13 $\frac{1}{2}$		9 41 8
1 *	4	17 cir.		10 cir. S.	
2 *	4	19 42		1 N.	
3 *	4 23 10	23 26—	23 40 $\frac{1}{2}$	11 $\frac{1}{2}$ S.	

January 9. from equal altitudes, the sun passed the meridian at 18 H. 17 M. 29 S. and January 10, at 18 H. 20 M. 50,6 S. per clock, which loses 59 S. upon the rate of fidereal time in one revolution of the fixed stars: therefore the sun may be computed to have past the meridian, January 8, at 18 H. 14 M. 7 S. .

\mathcal{D}	February 3			
a	4 49 31	49 45 $\frac{1}{2}$	50 0	{ The minute not noted, but from following observations must have been 50 M. to middle wire.
b	4 50	17	32	
\mathcal{D}	4 53 23 $\frac{1}{2}$	53 59	53 54—	
				7 44 28
b	5 0 6 $\frac{1}{2}$	0 21	0 36—	
\mathcal{D}	5 3 42	3 57	4 12	7 54 44

\mathcal{D}	February 5				
\mathcal{D}	5 0 0 $\frac{1}{2}$	0 16	0 31 $\frac{1}{2}$		7 42 52
*	5		10 13	6 N.	
\mathcal{D}	5 44 12 $\frac{1}{2}$	44 28 $\frac{1}{2}$	44 44—		8 26 56
*	5 52 40	52 55	53 10	5 N.	
\mathcal{D}	5 57 7—	57 23—	57 38		8 39 48
*	6 5 12+	5 27 $\frac{1}{2}$	5 43		
\mathcal{D}	6 22 24	22 39	22 55		9 5 0
*	6	30 3	30 18		

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O	1st Wire	2d Wire	3d Wire	Stars N. or S. of D's centre	D's Limb at Mid. Wire
	H / "	/ "	/ "	/	Apparent Time H / "
	February 7				
D	6 36 5	36 20	36 35		9 10 29
g	6	51 1/2 cir.			
i	6 58 48		59 17 1/2	7 N.	
l	6 59 49+	0.4		12 N.	
	7 1 24			12 S.	
D	7 24 4-	24 19	24 34		9 58 20
l	7	39 cir.		13 S.	
q	7 48 2	48 17	48 32	3 1/2 S.	
	8 2 7	2 22	2 36+	5 3/4 S.	
D	9 7 10	7 25	7 40		11 41 8
a	9		16 1+	10 S.	
b	9		18 20		
c			18 32		
h	9 26 3	26 17 1/2	26 32	7 1/2 S.	
k	9		27 46		
l	9	28 38-	28 52 1/2	8 1/10 N.	
m	9	32 15			
n	9 32 18	32 32	32 46 1/2		
o	9 36 42	36 57+	37 11+	6 S.	
p	9 38 29	38 43 1/2	38 58+	6 N.	
q	9 42 27+	42 42	42 56 1/2	5 3/4 N.	
D	10 9 38 1/2	9 53 1/2	10 9-		12 43 26
c	10 18 51	19 6	19 20+	14 N.	
f	10	26 32		0	
k	10	28 21			
l	10	29 27		22 N.	
q	10 43 16	43 31	43 46	18 2/3 N.	
D	10 51 34 1/2	51 49 1/2	52 4+		13 25 15
a	10 57 8-	57 23		13 3/4 N.	
f	11	7 17+	7 32	9 1/2 N.	

1st Wire

	1 Wire	2 Wire	3 Wire	Stars N. or S. of D's centre	D's Limb at Mid. Wire
	H " "	" " "	" " "	"	Apparent Time H " "
D d	12 22 10 12 29 16	22 25 + 29 31 —	22 40 + 29 45 +	12 $\frac{1}{10}$ S.	14 55 36
D d e	12 36 17 + 12 42 54 $\frac{1}{2}$ 12	36 32 $\frac{1}{2}$ 43 9 44 9 —	36 48 — 43 24 — 44 23 $\frac{1}{2}$	9 $\frac{1}{10}$ S. 12 S.	15 9 44
D d e	12 53 10 12 59 13 — 13 0 12	53 26 — 59 27 0 27 —	53 40 + 59 41 $\frac{1}{2}$ 0 41		15 26 31
D d	13 7 27 13 12 58 $\frac{1}{2}$	7 42 13 13	7 57 13 28		15 40 45
D e	13 21 32 13	21 48 — 27 48 +	22 3	3 S.	15 54 48

February 9

3 59 42	59 58	0 13 $\frac{1}{2}$	6 26 26
4	17 cir.	11 S.	
4	19 42	1 N.	
4 23 10	23 26 —	23 40 $\frac{1}{2}$ 11 S.	

The observations from February 3^d were made with my own clock, with which Mr. Dixon returned from the Cape of Good Hope December 30th, 1761, after examining the going of it there. He found it to get there 36,6 seconds upon sidereal time in one revolution of the fixed stars, or 29,3 seconds *per* day more than it got at St. Helena with the same length of pendulum: but I propose to give a more particular account of these, and some other experiments then made by Mr. Dixon at the Cape, some other opportunity.

From equal altitudes, the sun passed the meridian, January 30th, at 20^h 51' 8" $\frac{1}{4}$; February 5th, at 21^h 16' 5" $\frac{1}{6}$; and February 7th, at 21^h 24' 17" $\frac{1}{7}$. Hence the clock appears to

have got at the rate of $6''{,}3$ upon sidereal time in 24 hours. By the setting of four stars behind the hill, observed with the telescope of the equal altitude instrument, January 29th, and again February 7th and 8th (after the manner described by Mr. Mason in his account of the going of Mr. Ellicott's clock determined by him and myself in this manner, *Phil. Transf.* Vol. LII. Part II. Page 534.) the clock appeared to get $6''{,}25$ upon sidereal time in one revolution of the stars, which agrees exactly with the former determination by the sun's equal altitudes. In like manner, I always found the going of the clock, determined by these two different methods, would come out as nearly the same as the equal altitudes of the sun could be depended upon, that is to say, to a second, even from the observations of two successive days.

I must not pass by this occasion, without taking notice of some remarks, which Mr. Short passes on my method of examining the going of the clock, by observing stars setting behind a hill, with the telescope of the equal altitude instrument; (vide Mr. Short's account of Mr. Mason's paper concerning the going of Mr. Ellicott's clock at St. Helena. *Phil. Transf.* Vol. LII. Part II. Page 540). Mr. Short represents Mr. Mason, as saying in his paper, that I proposed making use of the equal altitude instrument to determine the regularity of the motion of Mr. Ellicott's clock, by observing the vanishing of the stars out of the field of the telescope, an expression not contained in Mr. Mason's paper, who is only speaking of our observing stars setting behind a hill, at the distance of a quarter of a mile, in the same part of the field of the equal altitude instrument. Had we proceeded in the method supposed in the remarks, no doubt the observations would have been liable to considerable inaccuracy: but as we used the telescope of the equal altitude instrument, only to assist the sight in observing the stars setting behind the hill, we were liable to no other error than what might arise from the small alterations of the instrument, arising from the changes of heat and cold, moisture and dryness, seen from the distance of the top of the hill, which will easily be allowed to be quite insensible. And, indeed, how otherwise could the observations, contained in Mr. Mason's paper, agree so well together as they do? A circumstance alone sufficient to create a suspicion of the objection being ill grounded. The reason of Mr. Mason and myself always observing the stars to vanish ~~behind~~ behind the hill, in the same part of the field of the telescope (that
is,

is, very near its centre) was, in order to keep the object glass at the same height; though this being less than an inch in diameter, and consequently subtending less than $13''$ from the top of the hill, there could not have been a second of time difference, whether the stars had been observed to vanish behind the hill, either in the upper or lower part of the field of view.

Mr. Short also remarks, that no inference can be formed with respect to the different forces of gravity, in different latitudes, from experiments made with clocks, because the same clock, set up on different sides of the same room, will be found to differ considerably from itself. I readily allow that, if clocks are fixed up in a slight manner, or against common wainscots, the experiments made with them cannot be depended upon. Nevertheless it does not appear, but that when they are fixed in a firmer manner, they may be depended upon near enough to be of a considerable use in physical enquiries: which I have reason to think from the many experiments I have tried with the Royal Society's clock, made by Mr. John Shelton, which I propose to give a particular account of at some other opportunity.

Observations of the Sun's setting in the Sea.

At the Observatory at the Alarum-House, which, by careful mensuration, I found to be elevated 1983 feet above the level of the sea. Therefore the height of the eye is 1988 feet.

1761	App ^t Time			
	H	'	"	
June 14	5	39	$5\frac{1}{2}$	☉'s upper limb set in the sea.
June 16	5	39	1	☉'s upper limb set. A little cloudy.
July 18	{	5	39	☉'s L. L. set in the sea, certain to 2 or 3 S.
		5	42	☉'s U. L. set in the sea, certain to $\frac{1}{2}$ S.
July 30		5	44	☉'s U. L. set in the sea. A little cloudy.

The like Observations made in James's-Valley, near the Sea-side.

1761		App ^t Time				
		H	'	"		
Nov. 16	{	6	23	42	☉'s L. L. fet	} Height of eye above the sea 16 feet.
		6	25	22	☉'s centre fet	
		6	26	21	☉'s U. L. fet	
Dec. 15	{	6	29	24	☉'s L. L. fet	} Height of eye above the sea 15 feet.
		6	32	10	☉'s U. L. fet	
Dec. 16		6	32	11½	{ ☉'s U. L. fet, thro' a small apperture in a cloud. The height of eye above the sea 15 feet.	
Dec. 17	{	6	29	24	☉'s L. L. fet	} Height of eye above the sea 15 feet.
		6	30	52½	☉'s centre fet	
		6	32	20½	☉'s U. L. fet	

8 August 5th

App^t Time

H	'	"	
5.	6.	7	{ A star 6 th magn. in Pisces, at centre of equal altitude instrument.
5.	6.	32	Jupiter's centre at the horizontal wire.
5	7	8	Jupiter's centre at the vertical wire.
The telescope remained fixed during these observations.			

Again.

17	31	30	The star at the vertical wire.
17	31	46	The star at the horizontal wire.
17	32	11	Jupiter's centre at the horizontal wire.
17	32	37	Jupiter's centre at the vertical wire.
Here also the telescope remained fixed during the observations.			

8 August 19th. Found the little star, which is 14" N. of Scorpiu, to precede it one second of time, by my parallactic wires

wires, with my watch, which makes four beats in a second of time. If any thing the difference was something more than a second of time: the little star may therefore be supposed to precede β Scorpii $17''$ in right ascension. By observations made with the 10 foot sector on several nights, while β Scorpii and the little star were passing the field of the telescope, I found the little star to be exactly $14''$ N. of β Scorpii in declination. For June 24 the difference was $12'',5$. July 21, $13'',6$. July 22, $14'',7$. July 23, $15'',3$: August 14, $12'',9$. August 17, $14'',8$.

The bright star in the foot of the Centaur, marked α in the catalogues, when viewed through a telescope, becomes divided into two stars, one of which is about the second, and the other about the fourth magnitude. They were both observed by the Abbé de la Caille. I found their distance, by the divided object glass micrometer fitted to the reflecting telescope, to be $15''$ or $16''$. But it is, in a manner, impossible to measure the distance of two stars very accurately with this micrometer, for being similar lucid objects, when they are brought very near each other, their light will be confounded together before they exactly coincide.

The larger Magellanic cloud, viewed through a telescope, exhibits a few stars which then appear separated to a considerable distance from each other. Their being so few in number, and so thinly scattered, is the reason of this phenomenon appearing so very faint.

The less Magellanic cloud, viewed through a telescope, exhibits a very remarkable lucid nebula, with some tolerable bright stars surrounding it. The nebula appears nearly circular, about $3'$ in diameter.

Transits of stars, with which the moon was compared, February 7, 1762, at St. Helena, taken upon the meridian, at the Royal Observatory, in the beginning of the year 1763, for ascertaining the exact difference of their right ascension. Vide page 363.

		1 Wire	2 Wire	3 Wire	4 Wire	5 Wire	
		' "	' "	H ' "	' "	' "	
January 16	a	"	52 52	8 53 31	54 21		of Cancer
	c	54 41	55 20+	8 55 59+			
	d		57 17	57 56	58 35	59 14	
	e	57 38	58 16½	58 56	59 35	0 13½	
	b	2 42::	3 21::	9 4 1:			of the Lion
	f					4 44	
	l		5 40+	6 20	6 59	7 38½	
	o			9 13 39:			
	q	19 5	19 44+	9 20 23½	21 3	21 14½	

Note, the clock gets five seconds per day upon fidereal time.

March 6	c		52 39	8 53 18	53 57	54 36
	b			9 1 19	1 58½	
	i				2 42	
	l				4 18	4 57
	m				7 34	8 14
	q			17 43—	18 22	19 1

Clock loses one second and an half per day.

March 7	a		50 10	8 50 49+	51 29	
	c	51 59	51 37+	53 17	53 56+	
	d		54 34½	55 13½	55 52½	56 31
	e		55 33½	56 14	56 52½	57 31
	b			9 1 17½		
	l		2 58	3 37	4 17	4 55½
	q		17 2	17 41	18 21	19 0

March 8	a			8	51 27	
	c	51 57	52 37	53 15		
	d	53 54	54 32½	55 12	55 51	56 29½
	e	54 54	55 33	56 12	56 51	57 29½
	i	0 41		9 2 0	2 39	3 18
	l				4 15+	4 53
	q	16 21	17 0	17 39½	18 19	18 57½

1 Wire

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		1 Wire	2 Wire	3 Wire	4 Wire	5 Wire
		' "	' "	H ' "	' "	' "
March 9	a	49 28	50 7	8 50 45 $\frac{1}{2}$	51 25	
	c	51 55+	52 34	53 13—		
	d	53 52 $\frac{1}{2}$	54 31	55 10	55 49	56 27
	e	54 52 $\frac{1}{2}$	55 31	56 10	56 49	57 28
	f			9 0 41		
	h			1 14+	1 55:	
	l		2 54 $\frac{1}{2}$	3 34	4 13 $\frac{1}{2}$	4 52
	n			7 30		
	o			10 54:	11 34:	
	q	16 20—	16 59	17 38	18 17+	18 56
March 12	a			8	51 52	52 31
	c	52 23	53 2	8 53 41	54 20 $\frac{1}{2}$	
	d		54 59	55 38	56 16 $\frac{1}{2}$	
	e		55 59	56 39	57 17	57 55
	g			9	2 5	
	l					5 19
	q		17 26—	9 18 6	18 45	19 24
March 13	a		50 33—	8 51 12	51 51+	52 30 $\frac{1}{2}$
	c	52 22	53 0.	53 40		
	d	54 19	54 57+	55 36 $\frac{1}{2}$	56 15 $\frac{1}{2}$	
	e	55 19	55 57	56 37 $\frac{1}{2}$	57 15+	57 54
March 15	a			8 51 11	51 50	52 29
	c	52 21	53 0+	53 39		
	d		55 57	56 36		
	e		56 57	57 36 $\frac{1}{2}$		
	q	16 46	17 25	9 18 4+	18 44	19 22+

Clock loses $\frac{1}{2}$ second per day.

April 6	a			8 50 54	51 33	
	c	52 5	52 43	53 22		
	d	54 1	54 40	55 19	55 58	
	e	55 1	55 40	56 19	56 58	
	g		59 25	9 1 7		
	i	0 48 $\frac{1}{2}$:	1 27	2 7		
	q	16 28	17 7+	9 17 47	18 26	19 5 $\frac{1}{2}$

Clock loses $\frac{1}{2}$ second per day.

N. B. By a mean of several transits of stars observed about this time, as well as by the foregoing observations, it appears that the intervals of the four first wires are exactly equal, and that the interval of the two last wires is smaller than the others by $\frac{1}{2}$ th part, which answers to $\frac{1}{2}$ th of a second of time in the transits of the above stars.

By several transits over the meridian, observed at the Royal Observatory, at the latter end of the year 1762 and beginning of 1763, the first or the preceding star of the three \star 's of Aquarius (with all which I compared the moon, December 4, 1761, at St. Helena) preceded the second or subsequent one $2'.7'',11$ of time in right ascension, and the third or last $3'.9'',70$ and the second preceded the third $1'.2'',59$ all according to the time of a clock regulated to agree with the diurnal revolution of the stars.

LVIII. *An Account of an extraordinary Disease among the Indians, in the Islands of Nantucket and Marthu's Vineyard, in New England. In a Letter from Andrew Oliver, Esq; Secretary of his Majesty's Province of Massachusetts's Bay, to Israel Mauduit, Esq; F. R. S.*

Boston, 26 Oct. 1764.

S I R,

Read Dec. 20, 1764. CONSIDERING your connexions, both as a member of the Royal Society, and of the Society for propagating the gospel among the Indians, I transmit you an account of an uncommon sickness, which prevailed the last year at the islands of Nantucket and Martha's Vineyard, which lie about six or seven leagues from each other, and the latter about four or five leagues distant from the Indian plantation at Mashpee on the Continent, where it did not make its appearance at all. As I had my account from the English minister, and from the physician at Nantucket, and from the society's missionary

fionary at the Vineyard, of each of whom I made the most scrupulous inquiry, you may depend on the truth of it.

About the beginning of August, 1763, when the sickness began at Nantucket, the whole number of Indians belonging to that island was 358: of these, 258 had the distemper betwixt that time and the 20th of February following, 36 only of whom recovered: of the 100, who escaped the distemper, 34 were conversant with the sick, eight dwelt separate, 18 were at sea, and 40 lived in English families. The physician informs me, that the blood and juices appeared to be highly putrid, and that the disease was attended with a violent inflammatory fever, which carried them off in about five days. The season was uncommonly moist and cold, and the distemper began originally among them; but having once made its appearance seems to have been propagated by contagion; although some escaped it, who were exposed to the infection.

The distemper made its appearance at Martha's Vineyard the beginning of December, 1763. It went through every family, into which it came, not one escaping it: fifty-two Indians had it, 39 of whom died; those, who recovered, were chiefly of the younger sort.

The appearance of the distemper was much the same in both these islands; it carried them off in each, in five or six days. What is still more remarkable than even the great mortality of the distemper is, that not one English person had it in either of the islands, although the English greatly exceed in numbers; and that some persons in one

family, who were of a mixt breed, half Dutch and half Indian, and one in another family, half Indian and half Negro, had the distemper, and all recovered; and that no person at all died of it, but such as were intirely of Indian blood. From hence it was called the Indian Sicknefs.

There had been a great scarcity of corn among the Indians the preceding winter: this, together with the cold moist season, have been assigned by some as the causes of the distemper among them. These circumstances, it is true, may have disposed them to a morbid habit, but do not account for its peculiarity to the Indians: the English breathed the same air, and suffered in some measure in the scarcity, with the Indians; they yet escaped the sickness. I do not see therefore, but that the Sudor Anglicus, which heretofore affected the English only, and this late Indian sickness, must be classed together among the Arcana of Providence.

I am,

S I R,

Your most obedient

Humble servant,

Andrew Oliver.

LIX. *Astronomical Observations made at the Island of Barbados ; at Willoughby Fort ; and at the Observatory on Constitution Hill, both adjoining to Bridge Town. By Nevil Maskelyne, A. M. Fellow of Trinity College, Cambridge, and F. R. S.*

[Read December 20, 1764.]

N. B. All the observations of the first satelite of Jupiter were made with a two foot reflector made by Mr. Bird, having an aperture of $3\frac{7}{8}$ inches in diameter, except where they are marked as made with my 18 inch reflector made by Mr. Short, having an aperture of four inches and an half in diameter. And the eclipses of the other satelites were all observed with the 18 inch telescope, except a few marked as made with Mr. Bird's two foot reflector. The latitude of Willoughby Fort is $13^{\circ} 5' N.$ and that of the Observatory $13^{\circ} 5' 15'' N.$ This agrees with the latitude I observed when our ship came to anchor at our first arrival in Carlisle Bay, which, carefully calculated, and reduced to St. Michael's Church, gives $13^{\circ} 5' 30''$ for its latitude. The Observatory is about 200 yards due east of the church, and Willoughby Fort is about 1600 feet distant from the same, bearing S. W. So that the Observatory is about $15''$ of longitude, or $1''$ of time to the east of Willoughby Fort. The latitudes of Willoughby Fort and the Observatory, set down above, result from altitudes of the sun taken with a bason of quick-silver by the brass Hadley's sextant made by Mr. Bird, being calculated from them, and the apparent time known from equal altitudes of the sun taken with the equal altitude instrument.

At:

App ^t Time.			
	H		
1763			At WILLOUGHBY FORT.
Nov.			
○ 13	15 5 38		Immersion of 1 st Satellite of Jupiter.
♀ 18	5 44 41		{ ○ set totally, or his upper limb set in the sea, height of my eye above the sea about 10 foot.
○ 20	{ 13 21 15:		Imm. 2 Sat. of Jupiter. Air hazy and moon-shine.
	{ 16 57 20:		Imm. 1 Sat. of Jupiter through thin clouds.
♂ 22	11 26 6		Imm. 1 Sat. of Jupiter.
♂ 24	13 52 29		{ Emerfion of ♀ Cancrī four magn. from the moon's dark limb, dubious to 5'', by taking my eye off the telescope for 10''.
Dec.			
♂ 1.	7 46 00:		{ Imm. 1 Sat. of Jupit. 18 inch telescope, magnifies 170. Air hazy; yet the satellite was separate from the body of Jupiter.
♂ 6	{ 12 10 45		Emerf. 3 Sat. of Jupit. 18 inch magnifies, 170.
	{ 17 21 40:		Emerf. 1 Sat. of Jupit. low towards the horizon. 18 inch telescope.
♂ 8	{ 10 24 55		Emerf. 2 Sat. of Jupit. } 18 inch { Sat. at a sensible.
	{ 11 46 35		Emerf. 1 Sat. of Jupit. } mag. 170 { Sat. at a very sensible distance from Jupiter's body.
♂ 15	{ 12 59 46		Emerf. 2 Sat. of Jupiter.
	{ 13 37 9		Emerf. 1 Sat. of Jupiter.
♂ 17	8 5 46:		Emerf. 1 Sat. of Jupiter. Air hazy.
♂ 21	14 12 0		{ A small telescopic star preceded Jupiter's centre 1.' 34'' in right ascension, and was 7'' N. of the same in declination.
♂ 22	{ 15 28 35		Emerf. 1 Sat. of Jupiter.
	{ 15 35 35::		{ Emerf. 2 Sat. of Jupiter. 2 foot telescope. Almost touched the 1 Sat. at the emerfion.
♂ 24	9 56 20		Emerf. 1 Sat. of Jupiter.

		At the OBSERVATORY.	
1764			
Jan.			
♂ 7	13 39 43:		{ Emerf. of 1 Sat. of Jupiter. Air a little hazy, but Jupiter sufficiently bright.
	{ 8 7 20		Emerf. 1 Sat. of Jupiter.
♂ 9	{ 8 31 18		{ A telescopic star (inserted in Senex's map of the Zodiac) in the line of the moon's horns produced, and $\frac{1}{2}$ a minute distant from moon's south horn.
	{ 10 2 8		Emerf. of 2 Sat. of Jupiter.
♂ 16	{ 9 59 38		Emerf. of 1 Sat. of Jupiter.
	{ 12 37 59		Emerf. of 2 Sat. of Jupiter.
♂ 18	{ 9 47 46		Imm. of 3 Sat. of Jupiter.
	{ 12 0 13		Emerf. of 3 Sat. of Jupiter.

App ^t Time		
1764	H	"
Jan.		
h 21	{ 10 59 28 11 23 18	<p> \propto Leonis (which was occulted by the \triangleright this night in Europe) precedes \triangleright's subsequent limb in right ascension by the parallactic wires of my 18 inch telescope. </p> <p> 2. 8. 30 2. 17. 46 </p>
\triangleright 23	11 52 36	Emerf. of 1 Sat. of Jupiter. Emerg'd instantaneously.
\S 25	6 20 51	<p> Emerf. of 1 Sat. of Jupiter. A good observation; though only 46' after sun set, sufficiently dark. </p>
Feb.		
\S 8	{ 9 43 24 10 9 19	<p> A star 6.7 magn. in Aries, in line of \triangleright's horns, produced 1' S. of \triangleright's south horn. Certain to 10''. </p> <p> Emerf. of 1 Sat. of Jupiter. </p>
\S 17	8 24 39	Occultation of ι Leonis 6 magn. by \triangleright 's bright limb, certain to 5''. Full moon this day.
\triangleright 20	{ 11 50 32 12 7 6 15 7 37	<p> The Virgin's spike precedes \triangleright's subsequent limb } 1° 51' 11'' </p> <p> in right ascension by parallactic wires. } 1 57 33 </p> <p> Ditto by transit instrument upon the meridian } 2 55 28 </p> <p> N. B. The Virgin's spike was occulted by the \triangleright this night in Europe. </p>
	{ 7 54 8	<p> Internal contact of one of Jupiter's satellites, with his east limb at its entrance upon his body. </p>
\mathcal{U} 23	{ 7 54 44 8 5 3 13 48 46	<p> Sat. disappears. Suppose it has been visible 36'' upon his body. </p> <p> Emerf. of 3 Sat. of Jupiter. </p> <p> Antares follows \triangleright's subsequent limb in right ascension } 8. 10. '' </p> <p> by parallactic wires of 18 inch telescope. </p>
\S 24	8 30 42	Emerf. of 1 Sat. of Jupiter.
March		
\S 6	7 5 12	Emerf. of 2 Sat. of Jupiter. Air very clear.
\mathcal{U} 8	7 2 43	Emerf. of ϵ Pleiadum from the \triangleright 's bright limb, certain to 10''.
h 10	{ 7 21 46 9 24 49	<p> β Tauri, 2 magn. precedes \triangleright's preceding limb in } 1.° 55' 28'' </p> <p> right ascension by parallactic wires. } 2 41 26 </p>
\triangleright 12	{ 9 49 22 ¹ 11 9 15	<p> Occultation of ν Gemini 5 magn. by \triangleright's dark limb. </p> <p> Emerf. of ditto from \triangleright's bright limb, certain to 10''. </p>
δ 13	{ 7 56 7 9 13 29	<p> Occultation of 2 ν Cancri by \triangleright's dark limb. </p> <p> Occultation of 3 ν Cancri by \triangleright's dark limb. </p>
\S 16	11 17 18	Occultation of σ Leonis 4.5 magn. by \triangleright 's dark limb.
h 17	6 41 28	Beginning of the eclipse of the \triangleright .
Eclipse of \triangleright	{ 7 26 11 7 36 29 9 26 50	<p> Shadow at middle of Mount Ætna. </p> <p> Shadow of the middle of Mount Hercules. </p> <p> End of the eclipse. </p> <p> N. B. I observed the beginning and end with an opera glass of Dollond's construction. </p>

App ^r Time		
1764	H	"
April		
6	8 28 11	Emerf. of 3 Sat. of Jupiter.
8	8 33 30	3 Sat. arriv'd to its greateft brightness.
13	{ 13 2 32 13 48 48	Imm. of β Virginis into γ 's dark limb.
15	7 19 17	Emerf. of ditto from γ 's bright limb, certain to 10".
		The Virgin's spike precedes γ 's preceding limb in } 49.' 43."
		right afcenfion by parallactic wires
N. B. It was occulted this night in Europe.		
May		
10	7 7 47	Imm. of σ Leonis 4.5 magn. into γ 's dark limb.
June		
6	{ 7 41 4 8 53 33	Imm. of α Leonis 4.5 magn. into γ 's dark limb.
14	11 13 14	Emerf. of ditto from γ 's bright limb. Certain to 5".
		τ Sagittarii 4 magn. precedes γ 's fubfequent } 1.° 8.' 46"
		limb in right afcenfion by parallactic wires.
July		
9	{ 9 51 56 13 22 27	Antares follows γ 's preceding limb in right afcen- } 3.° 8.' 48"
		fion by parallactic wires. } 1 32 c
12	{ 8 39 11 10 31 23	τ Sagittarii 4 magn. prec. γ 's prec. limb in right } 6 21 33
		afcenfion by parallactic wires. } 7 9 19
Aug.		
5	9 1 28	Occultation of π Scorpii 3 magn. by γ 's dark limb.
10	{ 7 41 43	Occultation of α Capricorni 6 magn. by γ 's dark limb, but
		which was fo near to being full, that the ftar feemed to van- nifh in the illuminated arch terminating light and darknefs.

Befides the above obfervations, I have taken a great many of the difference of right afcenfion between the γ 's enlightened limb and proper ftars (which I have not yet reduced) by means of parallactic wires in the focus of my 18 inch reflecting telefcop; from which, after making the requifite calculations, I make no doubt of being able to deduce the moon's horizontal parallax in that latitude, and thence, by proportion, the equatorial parallax of the moon with great exactnefs, which has never been done yet in fo direct a manner,

LX. *Farther Remarks upon M. l'Abbé Barthelemy's Memoir on the Phœnician Letters, containing his Reflections on certain Phœnician Monuments, and the Alphabets resulting from them. In a Letter to the Rev. Thomas Birch, D. D. Secretary to the Royal Society, from the Rev. John Swinton, B. D. F. R. S. Member of the Academy degli Apatisti at Florence, and of the Etruscan Academy of Cortona in Tuscany.*

Reverend Sir,

Read Dec. 13, 1764. **M.** L'Abbé Barthelemy's memoir on the Phœnician letters has again, with very large additions, been just (1) communicated to the learned world. Some at least of those additions have been made, as there is exceeding good reason to believe, if not very lately, several years after the memoir itself was read. This, as M. l'Abbé is said to be the first antiquary in France, and must undoubtedly have a great influence over the members of that illustrious body which he has so long adorned, cannot well fail of being considered by many people as a confirmation of the suspicion for some time

(1) *Mémoires de Littérature, tirés des Registres &c. Tom. XXX. p. 405-426. A Paris, 1764.*

entertained in several parts of Europe, and hinted at by me in (2) a former paper. It will therefore enable us to account for the late publication of a piece, which seems to have been cried up by M. l'Abbé's admirers as one (3) of the most valuable literary productions of the present age. What degree of attention to this performance from the lovers of antiquity is really due, I shall not at present take upon me to decide. My sentiments of it, however, if not yet sufficiently known, from the following short additional remarks, submitted with the utmost deference to the superior judgment of the Royal Society, will very clearly appear.

I.

M. l'Abbé still asserts, that צִרָא, TZORA, or TZVRA, in the first line of the Maltese-Phœnician inscription, [TAB. XXII.] denotes the city of Tyre. To which I shall only beg leave to reply, that this assertion is utterly repugnant to the testimony of the Tyrian coins; which constantly exhibit the word צִרָא, TZOR, or TZVR, as the name of that city. This is a fact expressly allowed by (4) M. l'Abbé himself, though he produces it in support of the notion here advanced; with which it must, even at first sight, be considered as altogether incompatible.

To the first letter of the next word he still likewise attributes the power of *He*, (5) and consequently affirms that word to be הֶוֶרְוִם, HOC VOTVM, THIS VOW.

(2) *Philosoph. Transf.* Vol. L. Par. II. p. 799. Lond. 1759.

(3) *Journ. des Sçavans*, Decembre 1760. p. 348. M. de Guign. *De l'Orig. des Chin.* p. 60. A Paris, 1760.

(4) *Mémoir. de Litter. &c.* ubi sup. p. 409.

(5) *Ibid.* p. 410, 413.

Two Transcripts of the Maltese-Phoenician Inscription.

9454 49 109 199 545 544 45
9449 49 109 199 545 544 45
0447 9449 109 199 545 544 45
5449 109 199 545 544 45

5449 9449 49 109 199 545 544 45
5449 49 109 199 545 544 45
45 109 49 109 199 545 544 45
5449

But as the absurdity of this notion was clearly demonstrated in my former remarks, to which no reply has yet been made, I shall take no farther notice of it here.

We are also told by this learned (6) author, that the first word of the second line was either עברן or עברנא, with the *Nun* and *Aleph* so closely connected as to form a kind of monogram; the faintest traces of which are, however, scarcely, if at all, to be seen.

That the word cut originally in the stone was עברו, FECERVNT, in my former remarks (7), I have rendered sufficiently clear. And that the two elements *Nun* and *Aleph* should be so confined as to occupy a space barely sufficient for one of them, will not be readily admitted by any person moderately acquainted with the manner of writing observed in the later Phœnician inscriptions. But to wave all other considerations relative to the point in view, that the verb here is expressed in the third person plural, the last word of the inscription, יברכס, BENEDICAT IIS, seems evidently to prove. 'Tis worthy observation, that M. l'Abbé represents Count Caylus's copy of the inscription as much superior in point of accuracy to Father Gori's; and yet, in determining, or rather attempting to determine, the form of the last letter of עברו, he apparently prefers the latter to the former. For he adduces Father Gori's copy in support, or rather confirmation, of the other. But such conduct as this, in M. l'Abbé's situation, is to me no great matter of surprize. It is plainly intended to serve a favourite hypothesis, which cannot be easily maintained.

(6) *Mémoire de Litter. &c.* ubi sup. p. 410.

(7) See above, p. 126, 127.

With regard to the proper name עבדאסר, ABDASAR, or ABDASARVS, I can by no means believe it to be the same with ABDASTARTVS. That those two words had not the same origin, seems to me, at first sight, self-evident. This is likewise confirmed (8) by a writer of great erudition. But the account I have (9) already given of the Phœnician name עבדאסר, ABDASAR, supported by the best authorities, will, I flatter myself, set this matter in the clearest light.

(10) I formerly observed, that ASERIMOR, ASERIMAR, or ASERIM-HAMMAR, was probably composed of ASERIM, or ASERYM, ΑΣΕΡΥΜΟΣ, the name of one of the kings of Tyre, according to Menander Ephesius, and מר, MAR, or rather דמר, HAMMAR, IPSE DOMINVS. But M. l'Abbé, (11) in the piece before me, takes it to be perfectly equivalent to the word ΑΣΕΡΥΜΟΣ itself; the Greeks seeming to him to have terminated in ΟΣ the Phœnician proper names ending in OP, as the other natives of Greece did several words used by the Lacedæmonians of the same termination. To which I shall beg leave to reply, that the Greek dialect of the Lacedæmonians was widely different from the Phœnician tongue; and consequently that all arguments drawn from their supposed agreement, or affinity, must be fallacious and inconclusive. Nor will the composition of the name אסר-דמר, or אסר-דמר, ASERIM-HAMMAR, viz. אש-רה-מר, ASHERAH-MAR, LV-

(8) Matth. Hiller. *Onomast. Sacr.* p. 590. Tubingæ, 1706.

(9) See above, p. 127, 128.

(10) See above, p. 129.

(11) *Mém. de Litter.* ubi sup. p. 410, 411.

CVS DOMINVS, which he exhibits to our view, afford a proper degree of satisfaction to any rational person engaged in such philological inquiries. Farther, it is obvious to every smatterer in the Greek language, that in the words ΕΙΡΩΜΟΣ, ΑΒΔΑΣΤΑΠΤΟΣ, ΑΣΕΠΤΜΟΣ, ΒΑΛΕΑΖΑΡΟΣ, ΒΑΔΕΖΩΡΟΣ, &c. HIROMVS, ABDAS-TARTVS, ASERYMVS, BALEAZARVS, BADEZORVS, &c. handed down to us by Josephus (12), from Menander Ephesius, ΟΣ is no part of the Phœnician names, but only a Greek termination superadded to them. The word ΑΣΕΡΙΜΑΡ therefore, or ASERIMOR, would have become, when adopted by a Greek, ΑΣΕΡΙΜΑΡΟΣ, or ΑΣΕΡΙΜΩΡΟΣ; not ΑΣΕΠΤΜΟΣ, as M. l'Abbé has been pleased to assert. So the Tyrian, or Phœnician, proper name 𐤇𐤓𐤍, HIRAM, or HIROM, as it occurs in Scripture, (1 King. ix. 12.) is rendered by the Septuagint and Josephus, after Menander Ephesius, ΕΙΡΩΜΟΣ, HIROMVS. But the most striking instances, or rather those directly in point, are ΒΑΛΕΑΖΑΡΟΣ, ΒΑΔΕΖΩΡΟΣ, or BALEAZAR, BADEZOR, when stripped of their Greek termination; with which ASERIMAR, or ASERIMOR, does most perfectly agree. This amounts to the strongest presumption, that M. l'Abbé's notion of the composition of that name is destitute of every support. Hence we may fairly conclude, that the account by me formerly given of the constituent parts of this word was strictly agreeable to truth; and consequently that the fourth element was *Mem*, and not *He*, as I then incontestably proved.

(12) Menand. Ephes. apud Joseph. *Cont. Apich.* Lib. I. p. 1043.

With

With respect to the four first letters of the third line, " they form a difficulty, says (13) this celebrated writer, embarrassing enough. The two last give " the word בן, BEN, FILIVS, SON; but this word " here ought to be read in the plural number. Was " not the plural number sometimes pointed out " amongst the Phœnicians by the addition of an *He* " and a *Nun*, in the same manner as the Chaldees " had an *Aleph* added to the beginning and the end " of this word also in the singular? Or rather did " not the two letters, *He*, *Nun*, forming the pro- " noun הן, IS, ILLE, give us to understand, that " Abdassar and Afferemor were brothers only by " adoption? I dare not decide in this matter, and " shall content myself with observing, that the dif- " ficulty regards only the language of the Phœni- " cians, of which we are totally ignorant, and by " no means the powers of the letters, which have " been sufficiently established in this memoir."

Here 'tis obvious, at first sight, that M. l'Abbé is not only embarrassed, but seems actually to sink under the weight of the difficulty he has himself proposed; as of this, notwithstanding his uncommon sagacity and penetration, he has not been able to supply his readers with an adequate solution. For 1. The word הנבן cannot be of the plural number, as he supposes, or rather positively asserts; both the correspondent Greek and the tenor of the inscription, unless I am greatly deceived, being utterly repugnant to such a supposition. 2. The Chaldee term he mentions being of the singular number, and absolutely dissimilar to the pretended Phœnician word,

(13) *Mém. de Littér.* ubi sup. p. 411.

הנבן ;

הנבן ; the adduction of it seems altogether impertinent, on the present occasion. 3. The pronoun הן is equivalent to the Latin *EARVM*, not *IS*, *ILLE*, as M. l'Abbé is pleased to affirm ; and being a suffix, or affix, adheres to the end, not the beginning, of a word. But if it should be taken for a separate pronoun, it answers to the Latin *ILLÆ*, not *IS*, *ILLE*, as it is rendered by M. l'Abbé. That Abdasar and Aferimar therefore were brothers only by adoption, is a chimerical notion, void of even the least shadow of rational proof. 4. He in effect declares the difficulty to be insoluble, when he owns himself incapable of deciding in this matter. 5. He makes the same declaration, when he asserts the difficulty to regard only the Phœnician language ; which he affirms to be utterly unknown, though both he and M. de Guignes have in express terms affirmed it to be almost intirely the same with the Syriac, and he has himself attempted to explain several inscriptions in it. Nor will it in the least avail him to refer the difficulty to the Phœnician tongue, or rather our ignorance of that tongue. For he undertook the interpretation of the whole inscription he has here so minutely considered, in order to deduce a Phœnician alphabet from it ; and unless he has, in some measure at least, effected this, how can he take upon him to ascertain the powers of the letters of which his alphabet is composed ? I would therefore, in my turn, beg leave to ask him the following question. Is it not more ingenuous, more liberal, and more worthy M. l'Abbé Barthelémy's (14) exalted merit, to retract an error

(14) *Journ. des Savans*, Aout 1760. p. 277. (1).

than to persist in it? To give up a point than to attempt the defence of it, when he seems even to look upon it himself as altogether indefensible?

The last word but one of the inscription M. l'Abbé represents by the Hebrew characters **מהמקל**, taking the second letter for *He*. He has likewise given us a pretty good account of **מקל**, the latter part of it; but has only just mentioned the particle **מה**, formed of the two first letters. And in this he has, perhaps, acted prudently enough. For none of the significations of that particle, at least none that I can find, will accord with the sense of that part of the inscription in which it occurs; whereas if we suppose the second element to be *Mem*, as I am fully persuaded it is, every difficulty will immediately vanish. This I have clearly demonstrated in my former remarks. I must beg leave farther to observe, as pertinent to the present occasion, that as a variety of winds will give a variety of directions to a ship's motion, any navigation effected by different winds may be termed crooked, any voyage performed by their assistance oblique. So that we cannot infer from either of M. l'Abbé's translations of the word in question, that the two Tyrians mentioned in our inscription were thrown upon the island of Malta by a tempest, as this learned author has been pleased to assert. The latter of those translations was owing, as he informs us, to "the favour of certain suppositions, which, for brevity's sake, he has suppressed."

From what has been said it appears, that the character denominated *He* by M. l'Abbé, and by me *Mem*, is an object of some importance, with regard to the explication of this inscription. If it be taken
for

for the former element, two or three parts of the monument are so involved that they are scarce, if at all, intelligible; if for the latter, the sense runs throughout unembarrassed, consistent, and clear. Nor does this character differ more from that allowed to stand for *Mem*, by M. l'Abbé, than do several of the acknowledged somewhat different forms of *Mem* from one another. A draught of it, however, seems not to have been yet published, by M. l'Abbé, that can be absolutely depended upon. For in the copy he first communicated to the learned world, represented by him as a transcript perfectly agreeing with the original, this character approaches a little nearer to his figure of *Mem* than it does in the first plate of of the memoir before me, and consequently the latter seems a little more favourable to his hypothesis. Whether this minute alteration, which is too inconsiderable to affect the point in question, ought to be attributed to the inattention or incapacity of the engraver, or to some other cause, I shall not take upon me to decide. How that matter really stands is best known to M. l'Abbé.

“ But the power of the letter *He*”, says M. l'Abbé, “ is fixed by other examples which I shall soon “ produce.” Now that a character representing *He* does not occur in the monument under consideration, has already, I flatter myself, been rendered sufficiently clear; whether or no it is to be met with on any of the coins produced in the paper before me, by M. l'Abbé, comes therefore next to be considered here.

II.

My explications of the first, second, and sixth medals in M. l'Abbé's plate of coins may be seen in a small Latin dissertation, put to the press at Oxford, in 1753. That of the sixth, however, which is a medal of Laodicea, must be owned to be incomplete; the four last letters of the inscription having been defaced on my coin, by the injuries of time. Nor am I intirely satisfied with M. l'Abbé's interpretation of the latter part of this inscription, as it seems very forced and unnatural, and even contrary to the faith of history. Nay, it seems not perfectly to please M. l'Abbé himself, as he has not absolutely settled the power of one of the letters of which it is composed; but contents himself with (15) observing, "that this slight difficulty will hereafter be removed by other monuments." His interpretation of the inscription exhibited by the coin of Sidon, which he makes coeval with the reign of Antiochus IV. is likewise liable to exception, as will appear to every one moderately versed in this branch of literature, who examines it with proper attention. The other two pieces of Sidon present nothing very remarkable to our view. One of them has nevertheless handed down to us the very character asserted by me to represent *Txade*, but taken by M. l'Abbé Barthelemy for *Thau*, immediately preceding certain numeral characters, which have been fully explained in one of my former papers, on the reverses of several Sidonian coins.

(15) *Mém. de Litter.* ubi sup. p. 417.

Now the letter *He* does not appear on any of these medals, and consequently nothing can be inferred from any of them in favour of the form of that element contended for by M. l'Abbé. On one of those coins, however, said by this learned antiquary to have been struck at Marathus, but which in reality ought not to be attributed to that city, now in my possession, the very same character occurs, with the power of *Mem*, that M. l'Abbé exhibits on two of the medals of Menæ as occupying the place of *He*. My explication of this coin, which I then took to belong to Marathus, was printed here, in 1753. But I afterwards observed, that the Phœnician inscription on this medal consisted of four letters, מרתב, the last of which was *Beth*; and that on all the similar medals, or draughts of them, which I had seen, four characters likewise appeared, the fourth of which was either *Beth* or manifestly a part of that element, not *Ajin* or *Ain*, as M. l'Abbé, without any manner of foundation, seems to imagine. Hence I concluded, that these pieces could never have been struck at מרת, MARATH, or MARATHUS, and therefore scrupled not a moment to explode my former opinion. To this I was farther excited by the numeral characters in the exergues of two of them, at present a part of my small collection of Phœnician coins; which, if I am not greatly mistaken, clearly point out the years of Rome 748 and 749. But about that time Marathus was either in ruins or entirely razed, and the territory appertaining to it occupied by the Aradians, according to Strabo (16).

(16) Strab. *Geograph.* Lib. XVI. p. 753. ^{Lutetiae} Parisiorum, 1620.

I therefore cancelled that part of my small work in which the interpretation of the inscription, preserved by one of these Phœnician medals, was contained. The cancelled part is, however, still in my hands

Of the Sicilian medals in M. l'Abbé's plate four are to be attributed to one city, and two to two others. Of the latter M. l'Abbé assigns one to Imachara, and the other to Carthage; with what truth, I shall not take upon me at present to decide. But that a person so justly celebrated for his knowledge of antient medals, particularly Punic and Phœnician medals, as is M. l'Abbé, should first ascribe the former to I know not what *Castra Cæcilia*, or *Castra Julia*, and afterwards to Panormus, now Palermo, is to me, I must confess, real matter of surprize. For the Punic name on these coins is evidently מַחְנַת, MAHHANOTH; MEHNOTH, or, as *Hbeth* is sometimes divested of even the force of an aspirate (17), MENOTH; which apparently answers to the Greek ΜΗΝΑΙ, and the Latin MENÆ, the name of a city in Sicily, called Menéo by Cluverius (18), several of whose medals adorn the cabinets of the curious at this day. Nay, one of the Punic coins of Menæ published by M. l'Abbé, though without any explication of the Punic inscription, and consequently without sufficient proof of the point in view, has been expressly attributed to Menæ, or Menéo, by Goltzius (19). An accurate description of a medal of Menæ, together with a complete interpretation of the Punic inscription it exhibits, may

(17) Bochart. *Phal. Lib.* I. c. i.

(18) Phil. Cluver. *Sicil. Antiq. Lib.* II. c. ix. p. 339.

(19) Hubertus Goltzius, in *Num. Sicil.* Tab. XII. num. 5, 6.

be seen in the Latin dissertation (20) above referred to, which it would be superfluous to touch upon here.

With regard to the Punic element taken for *He* by M. l'Abbé, on two Siculo-Punic coins, he seems to give up in one part of his memoir the form of it so warmly by him contended for in another. For he expressly allows, that this character on one of the Siculo-Punic medals exhibiting it may represent *Mem*, as well as *He*. From whence we may infer, that the correspondent letter on the other, as the word to which this character belongs is on both medals the same, may likewise, with no small appearance of truth, be taken for *Mem*; and consequently that, according to M. l'Abbé, the letter *He* may be supposed never in reality to have existed on either of those Siculo-Punic coins.

'Tis observable, that on one of the coins of *Menæ*, in M. l'Abbé's plate, the words קרר חרשר, VRBS NOVA, seem to appear; and that the first element of this inscription, if the draught of it here may be depended upon, is the *Koph* of nearly the Chaldee form. I have, however, a Punic medal in my collection, (see TAB. XXI.) with the old Phœnician *Koppa* on the reverse, and the names of two Sicilian cities, in Punic characters, never hitherto published. A galeated head, with a sprig of laurel before it, on one side, presents itself to our view; and the triquetra, or symbol of Sicily, with a human face in the middle of it, appears on the reverse,

(20) Swint. *De Num. quibusd. Samaritan. & Phœnic. &c. Dissert.* Oxon. 1753.

attended

attended by the words MAGEL, CAMIC, the Punic names of two Sicilian towns. The latter of these was the CAMICVS (21) of Diodorus Siculus and Herodotus, and the former the MACELLA, or MAGELLA, from whence the MAGELLINI of Pliny (22), of Dio (23) and Polybius (24). The letters of the inscription are drawn from the left hand to the right, contrary to the usual Punic manner of writing. But such accidental mistakes of the moneyers as this are sometimes visible on antient coins. This valuable medal, which formerly had a place in Lord Winchelsea's noble collection, corrects the famous Bochart, with respect to the origin of the name CAMICVS; and has preserved an unusual Punic form of *Lamed*, agreeing with a Samaritan (25) one of the same element, as well as a figure of *Ghimel*, that not seldom occurs on the Siculo-Punic coins. Some may, however, consider this medal as of Greek extraction; the third letter so nearly resembling *Lambda*, and the antient Sicilian Greeks, on certain occasions, having used the Phœnician *Koppa* upon their coins, as appears from the medals of Syracuse. But as the *Mem* is plainly Punic, or Phœnician, and both the Greek terminations wanting here, I can by no means prevail upon myself to subscribe to such an opinion.

(21) Diod. Sic. Lib. IV. & XXIII. Herodot. Lib. VIII.

(22) Plin. Nat. Hist. Lib. III. c. viii.

(23) Dio in Excerpt. apud Porphyrogennet. p. 637.

(24) Polyb. Lib. I. p. 24.

(25) Numism. Antiq. &c. à Thom. Pemb. & Mont. Gomer. Com. Collect. P. II. 85. num. 5.

As the Carthaginians therefore used, on certain occasions, the old Phœnician *Koppa*, or one of the earliest forms of *Koph*; we may from thence conclude, that the character (26) on a Punic coin by me long ago explained, so similar to that form, must undoubtedly be taken for the same element, as I then most clearly evinced. Nor has either M. l'Abbé, or any other French writer, hitherto overthrown this notion; though it has been called in question, if not denied (27); by M. Pellerin. Nay, it has been, in a manner, adopted some months since, in the *Journal des Sçavans*, (28) and even by M. l'Abbé himself, in the (29) celebrated memoir that is the object of my attention here.

It has been just observed, that the *Lamed* on the Sicilian coin last described is of an unusual form. Give me leave to add, that another of my Punic medals struck in Sicily has preserved a form of that letter, somewhat more similar to the correspondent character in the alphabet deduced by M. l'Abbé from several Siculo-Punic coins. This medal has on one side the head of Jupiter, and on the reverse two ears of corn, attended by the Punic inscription ALICA, or HALICAH, the AAIKTAI, or HALICYÆ, of (30) Diodorus Siculus, situated between Entella and Lilybæum, according to (31) Cellarius. The

(26) *De Num. quibusd. Samaritan. & Phœnic. &c. Dissert.* p. 86, 87. Oxon. 1750.

(27) *Récueil de Médailles de Peuples & de Villes, &c.* Tom. III. p. 141, 142, à Paris, 1763.

(28) *Journ. des Sçavans*, Aout 1763. p. 280.

(29) *Mém. de Litter. ubi sup.* p. 414.

(30) Diod. Sic. Lib. XIV. c. 25.

(31) Christ. Cellar. *Geograph. Ant.* Lib. II. c. 12.

coin has not hitherto been published, I believe, by any author whatsoever.

Amongst my Siculo-Punic coins there is likewise one with the letters *Hbeth* and *Beth*, on the reverse. Those elements undoubtedly form part of the word **חבלא**, *HIBLA*, or *HYBLA*, the Punic name of a town, or rather three towns, of Sicily, according to Bochart. That a mint was erected in one of them, at least, from an antient medal, with the words **ΥΒΛΑΣ ΜΕΓΑΛΑΣ** impressed upon it, may be certainly inferred. The Greek cities of Sicily not seldom exhibited on their money only part of their names, as we learn from (32) several of the Greek Sicilian coins; and that the towns there under the dominion of Carthage did the same, we have all the reason in the world to believe.

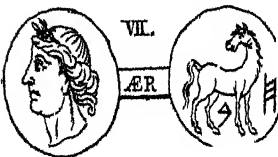
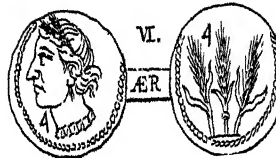
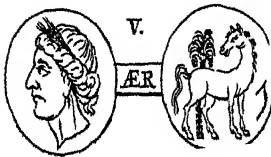
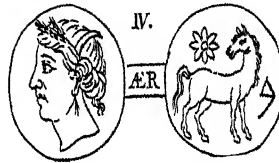
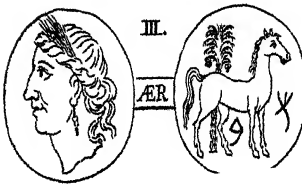
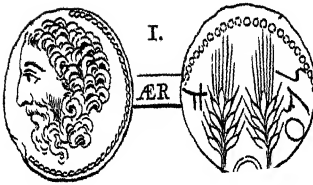
Another medal also appears in my little cabinet with the character representing *Hbeth* only, on the reverse. That character may be taken for the initial letter of the Punic proper name **חבלא**, *HIBLA*, or *HYBLA*; and may indicate this piece to have been struck in the city so called, as well as the former. Instances of such initial letters as this pretty frequently occur, on several of the (33) Greek Sicilian coins.

Two of my small Carthaginian medals have preserved the letter *Ghimel*, on their reverses, of the usual Punic or Phœnician form. This seems to be

(32) Erasmi Frœl. in *Notit. Elementar. Numism.* pass. Filippo Parut. in *La Sicil. descritt. con Medagl. &c.* pass. In Roma, 1649. Vid. etiam Hubertum Goltzium, in *Sicil. Numism.* pass. Antwerpæ, 1617.

(33) Erasmi. Frœl. in *Notit. Elementar. Numism.* p. 83. Viennæ, Pragæ, & Tergesti, 1758. Parut. et Goltz. ubi sup.

SICULO-PUNIC Coins.



the initial letter of GELA, the name of a town of Sicily, with several of whose (34) coins the cabinets of the curious are adorned.

Another of my Punic medals, struck in Sicily, presents to our view the two elements *Aleph* and *Beth*. These plainly point at the ABACÆNA of (35) Ptolemy, an antient town of Sicily; many remains of which, according to Cluverius, were some time since to be seen.

One of the Siculo-Punic coins in my small collection has handed down to us two alphabetic characters, the powers of which are not yet, perhaps, sufficiently known. The second of them, however, from the great resemblance it bears to the form of that element in the Maltese inscription, we may venture to pronounce *Tzade*; and the first, according to M. l'Abbé Barthelemy, occupies the place of *Aleph*. I am nevertheless rather inclined to believe, that it ought to be taken for *Koph*. This seems to appear from the draughts of several coins, published by (36) writers of good authority, as well as from the concurring sentiments of those writers themselves. If this notion should hereafter be adopted by the learned, the medal may be supposed to have been emitted from the mint at Catana, as *Tzade* sometimes in power answers to the Latin T; but if M. l'Abbé's opinion should be held more consonant to truth,

(34) Froel. Parut. & Goltz. ubi sup.

(35) Ptol. *Geograph.* Lib. III. c. 4. Phil. Cluver. *Sicil. Ant.* p. 386.

(36) *Veron. Illustrat.* Par. Terz. cap. sett. p. 258, 259. In Verona; 1732. Ridolfin. Venut. in *Saggi di Dissertaz. Accademich. pubblicamente lette, nella Nobil. Accadem. Etrusc. dell' antichiss. ma Città di Cortona.* p. 35. In Roma, 1735: Parut. *Lastanof.* &c.

from that at Afforus, the same element being likewise not seldom equivalent to the Latin S. On another of my Siculo-Punic medals, that letter somewhat resembles the *Kappa* of the Greeks. Admitting the second element to be *Tzade*, as I am fully convinced it is, both ASSORVS and CATANA may, with (37) great propriety, be considered as Punic proper names.

For the farther illustration of what has been here advanced, it may not be improper to exhibit draughts of the Sicilian medals just described, as well as of several others, now in my possession, adorned with Punic characters. From these (TAB. XXIII.), at first sight, will be deducible a Siculo-Punic alphabet [TAB. XXIV.]; which, though incomplete, may, perhaps, not prove unacceptable to the learned.

Before I conclude my remarks on the Siculo-Punic coins, published by M. l'Abbé, I must beg leave to repeat, that only two of them, which were struck at Menæ, are produced, in order to evince the reality of his form of *He*; and that he has himself allowed this character, on one of those medals, (which concession will likewise extend to that on the other, as has been already observed) to be as properly expressive of *Mem* as of *He*. I say, his form of *He*; because I cannot help taking the character he denominates *He* for *Mem*, as it so nearly resembles the common and usual form of that element, and as the sense it communicates even here, supposing it *Mem*, is so perfectly consonant to the tenor of such inscriptions. This will be readily admitted by every

(37) Val. Schind. *Lex. Pentaglot.* p. 113, 114, 1641. Hangvise, 1612.

one not totally unacquainted with the medallic science, and not an intire stranger to the first principles of oriental literature. For **מַעַם מְהֵנִי**, **EX POPVLO MENENIO**, **A POPVLO MENENIO**, **POPVLO MENENIO**, (scil. **CVSVS**, vel **PERCVSSVS**, **NVMMVS**) is certainly altogether as proper as **הַעַם מְהֵנִי**, **IPSE POPVLVS MENENIVS**, (scil. **CVDIT**, vel **PERCVSSIT**, **NVMMVM**) if not really much more so. That **s. c. i. e. SENATVS CONSVLTO**, **D. D. i. e. DECRETO DECVRIONVM**, **EX D. D. i. e. EX DECRETO DECVRIONVM**, &c. not seldom occur on antient coins, is a point too well known, (38) even to smatterers in antiquity, to be disputed amongst the learned.

III.

As my Latin dissertation upon the second Citiean inscription, of which I have already given a Latin translation, inserted in the **MARMORA** (39) **OXONIENSIA**, is almost ready for the press; I might easily excuse myself, at present, from taking any notice of M. l'Abbé's pretended explication of that inscription. But as this, in conjunction with his attempt to interpret the fourth, is undoubtedly by far the most exceptionable (40) part of his whole performance, since he seems totally to have mistaken the sense of both inscriptions; I shall beg leave to

(38) Lud. Debiel, è Soc. Jes. in *Utilit. Rei Numar. Vatez.* p. 99, 100, 112, &c. Viennæ Austriæ, 1733.

(39) *Marmor. Oxon.* Par. Secund. Tab. III. p. 7, Oxoniæ, 1763.

(40) *Mémoir. de Litter.* ubi sup. p. 421, 422, 423.

submit to the consideration of the Royal Society the following short strictures upon it, reserving to myself the liberty of being more explicit and particular on this head in my future dissertation.

1. With regard to the second Citiean inscription, the third letter of the first word is not *Mem*, but *Caph*; as most evidently appears from the autograph itself, now in the possession of the University of Oxford. Farther, that **דמנ** ought to be rendered DORMIAM, DORMIO, or JACEO, as M. l'Abbé asserts, had that been the first word of the inscription, as most certainly it is not, we shall scarce be able to learn from any lexicographer.

2. The sixth word of the first line is *Hbur*, *Chur*, or *Hur*, not *Chad*, as M. l'Abbé seems to imagine; the last letter of that word being manifestly *Resch*, and sufficiently distinguished by its length from *Daleth*, which occurs twice in the beginning of this line.

3. The term **מצבת** cannot bear a relation to any particular city, town, or country, as our learned antiquary is pleased to suppose; because this would imply, that the sepulchral inscription had only a single person for its object. But this notion is intirely overthrown by the word **גרותי**, in the second line, and **מתי**, in the third; both of which are evidently plurals in construction, and consequently cannot refer to less than two particular persons. Besides, as the most antient inscriptions were probably the shortest and most simple, the age of the monument itself seems to announce a duality of persons at least to have been pointed at by the inscription.

4. I can at present see no reason why M. l'Abbé should suppose this inscription to run in the first person, rather than the third. Perhaps he will say, that the Maltese inscription, as by him explained, affords us a remarkable instance of such an uncommon mode of expression. But this, I humbly conceive, is no reason at all; because in one particular inscription some peculiarities may appear, as is often the case, that in others do not occur. Besides, the postulate he begs, or rather assumes, will, I am persuaded, not be so readily granted him by the learned.

5. M. l'Abbé has added a fictitious *Schin* to the beginning of the second line, not the faintest traces of which are discernible on the stone. Nay, that *Schin* could never have been there, is self-evident, at first sight, from the very face of the inscription.

6. The word שלם, PAX, formed, according to M. l'Abbé, of the fictitious *Schin* and the two first letters of the second line, was most certainly never a part of this inscription.

7. He supposes an *hiatus* in the second line, and another in the third; whereas not a single letter is wanting in the inscription, nor any of the words so effaced as to be rendered illegible, by the injuries of time.

8. For the words מנב, מתי, לאמת, נם, תורת, עלם, למב, ח"י, על, משכב, שתת, אשתי, מתרת, שלם, בנח, which clearly present themselves to our view in the inscription, he has taken the liberty, to insert מתרת, אשתי, שתת, משכב, על, בנח, without the least shadow of a reason for such an arbitrary insertion; several of his letters being purely imaginary, and not the faintest traces of them having ever existed on the stone.

9. The

9. The name TAM, or, as he corruptly writes it, THAM, is taken by him for only part of a word; whereas it is a perfect and complete proper name, as most clearly appears from the face of the inscription, and from some antient writers of good authority, who will hereafter be referred to in these remarks.

10. He has not translated the words **בָּם, תָּרַח, עֵלָם, מִנֵּב, נִחַחִי, לְעֵלָם, כִּלָּא, מִחִי, לְאַמְרָא**, though they form so considerable a part of this monument; and though a translation of them is so essentially necessary, in order to arrive at a true interpretation of the inscription.

In confirmation of what has been here advanced, I shall beg leave to subjoin a short and concise explication of this inscription; which, I flatter myself, will not be found very remote from truth, as it is consonant to the faith of history, and supported by two or three Greek authors of very considerable note amongst the learned.

It may, however, be previously requisite to observe, that the plate hereunto annexed [TAB. XXV.] exhibits the most perfect representation of this monument, both with regard to the magnitude of the stone, and the forms and size of the letters, that can possibly be communicated to the learned world. As the copy therefore now presented to the public is the result of a fresh and most careful examination of the autograph itself, and by me, since the publication of the *MARMORA OXONIENSIA*, immediately deduced from it; this may be considered as the most accurate transcript of our Citiean inscription, in all respects, that has hitherto appeared.

The Oxford-Citæan Inscription.

.𐤀𐤁𐤂𐤃𐤄𐤅𐤆𐤇𐤈𐤉𐤊𐤋𐤌𐤍𐤎𐤏𐤐𐤑
 𐤒𐤓𐤔𐤕𐤖𐤗𐤘𐤙𐤚𐤛𐤜𐤝𐤞𐤟𐤠𐤡𐤢𐤣𐤤𐤥𐤦𐤧𐤨𐤩
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4 8 9 9 5 4 7

7 8 8 8 8 8 8

7 8 8 8 8 8 8

1. The first word, אַנַךְ, ANAC, or ONEC, seems to have denoted the same thing in Syriac (41) and Phœnician that ΟΝΥΞ did in Greek, and ΟΝΥΧ in Latin. But the same species of marble was denominated both ΟΝΥΧ and ALABASTRITES, as we (42) learn from Pliny, and other good authors. It cannot therefore well be doubted, but that אַנַךְ, ANAC, or ONEC, here may either be translated strictly and literally ALABASTRITES, or be rendered with sufficient propriety by the more general term MARMOR. The Cyprian stone itself, on which the inscription has been preserved, being a fine white alabaſter, or perfect ALABASTRITES, puts the point here insisted on almost beyond dispute.

2. I have already given so full and particular an account of the second word עֲבִירֶאסָר, which is a Phœnician proper name of a man, in my former remarks, (43) that it would be intirely superfluous and unnecessary to take any notice of it here.

3. That the third word, בֶּן, BEN, which occurs afterwards in the first line, is equivalent to the Latin FILIVS, I have (44), in a former work, rendered incontestably clear.

(41) Johan. Buxtorf. Lex. Chaldaic. & Syriac. p. 25. Bafileæ, 1622.

(42) Plin. *Nat. Hist.* Lib. XXXVI. c. 7, 8. Lib. XXXVII. c. 5, 6. Hor. *Carm.* Lib. IV. od. 12. Martial. *Epigram.* Lib. VII. ep. 93. Dioscorid. Lib. V. c. 153. Isidor, Lib. XVI. c. 15.

(43) See above, p. 127, 128.

(44) *Inſcript. Cit.* p. 22. Oxon. 1750.

4. The fourth word, עבדסם, ABDESVSIM, or ABDESASIM, is another Phœnician proper name of a man, not to be met with in any antient author. If we read it ABDESVSIM, it answers to the Latin SERVVS, or CVLTOR, EQVORVM; if ABDESASIM, (45) to REGENTIVM (viz. DEORVM Mundum REGENTIVM) CVLTOR, or SERVVS. The latter seems to be by far the most eligible lection. Which if we admit, it will seem to follow, that the Phœnician term סם, SASIM, was sometimes at least equivalent to DII, SVPERI, or rather DII Mundum REGENTES, amongst the Latins.

5. The sixth word, חר, HHVR, or HVR, is also a Phœnician, as well as a (46) Biblical Hebrew, masculine proper name. The first letter, *Hbeth*, here is of a pretty unusual form. I have another Phœnician inscription, never hitherto published, with the very same character in it; of which, if God grants me life and health, I intend to communicate an interpretation to the learned world.

6. The seventh term מצנר, from יצב, ought to be rendered in Latin LAPIS (47) SEPVLCHRALIS. It seems to begin a new sentence, and to be intirely detached from the preceding words. The genuine

(45) Vid. Jac. Gol. *Lex. Arabic.* in voc. ساس, five סם, REKIT PRO ARBITRIO. Sam. Bochart. *Hierozoic.* p. 16. Franequeræ, 1690. Jo. Leonhard. Reckenberg. *Lex. Hebr.* p. 1072. Jenæ, 1749.

(46) Matth. Hiller. *Onomast. Sacr.* p. 99, 789. Tubingæ, 1706.

(47) Leonhard. Reckenberg. ubi sup. p. 690. Jenæ, 1749. Christian. Stocki *Clav. Ling. Sanct. Vet. Test.* p. 470. Jena, 1727.

signification of this word is consonant enough to the tenor of the inscription, which will not admit the sense assigned it by M. l'Abbé. The figure of the *Tzade*, which is a very uncommon one, occurs in the inedited inscription I hope hereafter to explain.

7. The eighth word, לֵמֶב, LEMB, or LEMEB, is probably another Phœnician proper name. That it was used as a proper name in Syria, from Josephus is (48) abundantly clear. It occurs also as the proper name of a man in Festus. The substantive בֶּן, BEN, FILIVS, does not follow לֵמֶב, LEMB, with the father's name, according to the manner of writing not infrequently seen in the Phœnician inscriptions. But other instances of such an omission (49) as this have been observed, on several of the antient stones found in the ruins of Citium.

8. The verb וַיִּי next presents itself to our view, and ought to be rendered VIXIT, according to the Hebrew lexicographers. A suppression of the relative pronoun אֲשֶׁר, QVI, is observable here. But such ellipses as this were antiently not uncommon, according to Noldius (50).

9. The following character, though in a manner the same with the Citiean form of *Jod*, is likewise somewhat similar to the Palmyrene numeral character representing TWENTY, (51) and not diffi-

(48) Joseph. *Antiquit. Judaic.* Lib. XLII. c. 15. p. 599. Ed. Hudson. Oxon. 1720. Sex. Pomp. Fest. Lib. XVI. p. 455. Amstelodami, 1699.

(49) *Inscript. Cit.* 17, 21, 23, &c.

(50) Christian. Nold. *Concordant. Particular. Ebraeo-Chald.* p. 102. Jenæ, 1734.

(51) *Philosoph. Transf.* Vol. XLVIII. Tab. XXIV. Inscript. III.

milar to one of the equipollent characters used (52) at Sidon. As the word וִיִּי, VIXIT, therefore manifestly requires a numeral, I have taken the liberty to translate this TWENTY. The learned world may expect a farther account of this numeral in my future dissertation.

10. The next character sufficiently resembles (53) the Palmyrene form of *Schin*, on a Parthian coin by me formerly explained, as well as in the Palmyrene alphabet. It may therefore pass for that element, though it is somewhat longer; especially, as the Syrians were neighbours to the Phœnicians, and the letter *Schin* here begins a word so consonant to the tenor of this part of the inscription. That word is apparently שְׁנַיִם, which comes very appositely after וִיִּי, VIXIT VIGINTI, or rather QVI VIXIT VIGINTI. As it appears here in construction, it is a singular; but must nevertheless be translated ANNOS, the genius of the Phœnician language in this respect being probably the same with that of the Hebrew. The *Aleph* is to be considered as a *mater lectionis*, in the term before me; the letters *Aleph*, *Vau*, *Fod*, (54) not infrequently occupying the places of vowels amongst the earlier Hebrews, and consequently, as there is great reason to believe, amongst the Phœnicians also. But of this more hereafter.

11. The Phœnician substantive עֶלֶם, as in Hebrew, is undoubtedly equivalent to SECULVM, ÆTERNITAS, DURATIO HOMINIBVS ABSCONDITA, &c.

(52) *Philosoph. Transf.* Vol. L. Tab. XXXII. p. 805.

(53) *Philosoph. Transf.* Vol. XLIX. Tab. XVIII. p. 593.

(54) Campeg. Vitring. *Observat. Sacr.* p. 186.

It frequently occurs, both in a limited and unlimited sense, in the Old Testament; and accommodates itself, according to (55) Guffetius, to the nature of the subject to which it is applied. Which if we admit, when applied to men, it must denote the term or period of human life. And in this signification it (56) sometimes may be met with in holy writ, as it manifestly is in our inscription. The *Vau* here, in conformity to the Phœnician custom, is suppressed. But for a farther account of this word, recourse may be had to the learned Sontagius, in (57) his dissertation upon the terms **עולם** and **יום**, printed at Altorf, in 1695.

12. The Hebrew noun **מכאוב**, from whence **מכאובים** and **מכאובות**, is deduced from the (58) verb **כאב**, **DOLVIT**. By the extrusion of the two quiescent letters, *Aleph* and *Vau*, the substantive becomes **מכב**, (the same in pronunciation with **מכאוב**) as it appears in our inscription. Nor is it to be wondered at, that, before the invention of the vowel-points, the quiescent letters should have sometimes been suppressed, as they had in reality no power at all. Instances of such a syncope, or extrusion, as that here observed, are not seldom found in the sacred writings of the Old Testament. And that this was really the case with regard to the word **מכב**, the Chaldee term

(55) Jacob. Guffet. *Comment. Ling. Ebraic.* p. 1160, 1161. Lipsiæ, 1743.

(56) PSAL. LXXXIX. 1. CXIX. 44. & alib.

(57) Christoph. Sontag. in *Dissert. de עולם periodico & יום æterno*, Altorfii, 1695.

(58) Leonhard, Reckenberg. ubi sup. p. 738.

כבא, (59) agreeing perfectly in signification with it, seems incontestably to prove.

From what has been advanced we may conclude, that the Phœnician words **עלם מכב**, on our stone, are equivalent to *SECULI*, or *VITÆ, DOLORIS*, i. e. *VITÆ INFELICITER ACTÆ*, as I have taken the liberty to render them here.

13. The next word is **נחתי**, *DESCENDENTES*, or rather *DESCENDUNT*, the verb *SVNT* being suppressed, as amongst the Hebrews it frequently happened to be. The radix of this participle is **נחט**, *DESCENDIT*, a verb well enough known to the Hebrews, but frequently used by the Chaldees; who generally applied to it the signification of the verb **ירד**, as it prevailed amongst the Hebrews, according to (60) Buxtorf. It must be remarked, that **נחתי לעלם כלא** ought to be deemed the same expression as **עלם כלא נחתי**; the word **לעלם** having had different positions antiently assigned (61) it in a sentence by the Hebrews, and therefore undoubtedly by the Phœnicians. In the Chaldee several passages similar to this part of our inscription are to be found. So **היך נחתי גובא**, *SICVT DESCENDENTES IN FOVEAM sepulchri*, occurs, in *PROV. i. 12.* **נחתי ימא**, *DESCENDENTES AD MARE*, in *ISA. xlii. 10.* And in *PSAL. cxliii. 7.* we meet with **גוב בירח קבורתא עם נחתי**, *CVM DESCENDENTIBVS IN FOVEAM SEPUL-*

(59) Johan. Buxtorf. *F. Lex. Chald. Talm. & Rabbin.* p. 1001. Basileæ, 1639.

(60) Johan. Buxtorf. *F. Lex. Chald. Talm. & Rabbin.* p. 1330.

(61) F. Mar. de Calasi. *Concordant. Sacrer. Biblior. Hebraicpr.* in voc. **עולם**, p. 392-602. Lond. 1747.

CHRI; which seems to be a phrase of the very same import in Chaldee that the נַחְתִּי בְּלֹא, DESCENDENTES sunt, or rather DESCENDUNT, IN CARCEREM, of our inscription is in Phœnician. The participle נַחְתִּי, being in construction here, may, however, be considered either as a Chaldee, Hebrew, or Phœnician word. As for the terms לְעֶלְם בְּלֹא, IN ÆTERNUM CARCEREM, they are so obvious and clear that they require no farther discussion in this place.

14. The two following words, מְתִי לְאַמְתִּי, with the three preceding, form the third sentence, or part of the inscription. The first of them, מְתִי, deduced from מוֹת, MORI, EMORI, and in construction, is a participle converted into a noun, and may be translated MORTVI, or rather OCCISI, here. In this latter sense it occurs, in ISAÏ. xxii. 2. It may also be rendered HOMINES, or VIRI INSIGNES, according to Cocceius. But the former seems to be the most obvious and natural signification. The second of these words, לְאַמְתִּי, is undoubtedly the name of Amathus, a celebrated city of Cyprus, that was governed by it's own princes for a considerable period of time. The particle לְ points out the genitive case in our inscription, as it does in the proper names לְצִדֹן, LETZIDON, לְצֹר, LETZOR, or LETZVR, on the Tyrian and Sidonian coins. The same thing may sometimes be said of this particle, when the word immediately preceding it is in construction, (62) as we find manifestly to be the case here. From the inscription before me it appears, that the Phœnician name of Amathus was אַמְתִּי, AMATH,

(62) Vid. Reckenberg, ubi sup. p. 828.

not חמַת, HAMATH, or CHAMATH, as it has been written by Bochart (63).

15. The next word, בַּם, apparently to be deduced from the obsolete verb בָּם, ALTVM EST, ELEVATVM EST, &c. in the infinitive mood בּוֹם, from whence בָּמָה, EXCELSVM, may with sufficient propriety be interpreted MONVMENTVM SEPVLCHRALE; as בָּמָה, a word of the same origin, has actually been interpreted by (64) Schindler and Clodius. Farther, בַּם may be considered as no other term than the Syriac בַּיִם, SVGGESTVM, TRIBVNAL (65), TVMVLVS, &c. the excision of *ʿod* being common, as in צֹרֶן, for צִירֶן, and צֹרֶם, for צִירֶם, amongst the Phœnicians. Nor is the signification assigned בַּם here more consonant to the true import of the radix, from whence it is derived, than to the tenor of that part of the inscription to which it belongs.

16. That the Phœnicians wrote the Hebrew word מִירָה, STRVCTVRA, or rather STRVCTVRA ORDINATA, תִּרְה, and in construction תִּרְת, cannot well be denied; since מִירָה was no other (66) term than תִּר, or תִּרִּי, and it was common with the Phœnicians to expunge *ʿod*, as has been just observed. The verb substantive, in conformity to the Hebrew and Phœnician custom, has been apparently suppressed here.

(63) Sam. Bochart. *Chan. Lib.* I. c. 3.

(64) Val. Schind. *Lex Pentaglot.* p. 171. Hanovizæ, 1612.
Jo. Chriff. Clod. *Lex. Hebraic. Select.* p. 72. Lipsiæ, 1744.

(65) Johan. Buxtorf. *Lex. Chaldaic. & Syriac.* p. 54. Basileæ, 1622.

(66) Reckenberg. *ubi sup.* p. 598, 1563.

17. For

17. For בִּית the Carthaginians wrote בִּר, without *Jod*, as we learn from the Punic inscription explained in (67) a former paper. And that the Phœnicians wrote this word in the same manner, considering their frequent omission of *Jod*, I see not the least reason to doubt. The Ethiopians in this term most certainly never made use of that element. In our inscription it is to be therefore considered as equivalent to DOMVS, GENS, FAMILIA, &c. in which signification it not seldom occurs; sometimes being applied to a whole family, and at other times to a part of a family only, in the (68) sacred writings of the Old Testament. The three last words of the inscription, תָּאִם בֶּן עַבְדִּמֶלֶךְ, TAMI FILII ABDEMELECI, are so intirely clear of all difficulties, and so obvious to every one in the least acquainted with antient history, and oriental literature, that it would be superfluous to expatiate upon them here.

It may, however, not be improper to remark, that the use of the proper name ABD'ALMALEC, the same with ABDEMELEC, was retained by the Arabs long after the first appearance of our inscription. For the Khalîf Abd'almâlec, who succeeded Merwân I. above half a century after the death of Mohammed, departed this life (69) in the year of the Hejra 86, or of CHRIST 705. Nay, 'tis more than probable, that the same name prevails amongst the Arabs even at this day.

(67) *Philos. Transf.* Vol. LIII. p. 275, 276.

(68) Reckenberg, ubi sup. p. 130.

(69) Al Makîn, Greg. Abu'l Faraj, Eutych. Ism. 'Abul'fed. Ebn Al Athîr, aliique scriptor. Arab.

18. If what has been here advanced should meet with the approbation of the Royal Society, the following Latin and English versions of this inscription, which has so well escaped the injuries of time, may not prove unacceptable to the learned.

אֵן עֲבֵדְאָסָר בֶּן עֲבֵדְאָסִימ בֶּן חֹר—מִצֵּבָה
לְמֵב חִי כִּשְׁנָאֵת עֹלָם מִכֵּב—נִחַתִּי לְעֹלָם כֹּלָא
מִתִּי לְאַמְת—בֵּם תִּרְתָּ בֵּת תָּאם בֶּן עֲבֵדְמֶלֶךְ

MARMOR ABDASARI FILII ABDESASIMI FILII HHVRI—LAPIS SEPULCHRALIS LEMBI (vel LEMEBI) QVI VIXIT VICENOS ANNOS SECVLI DOLORIS (i. e. ÆTATIS sive VITÆ INFELICITER ACTÆ)—DESCENDVNT IN ÆTERNVM IN CARCEREM sepulchri MORTVI hi AMATHVNTIS (seu potius OCCISI hi AMATHVSII)—MONVMENTVM STRVCTVRA est DOMVS (vel FAMILIÆ) TAMI FILII ABDEMELECI.

THE MARBLE (or MARBLE TOMB-STONE) OF ABDASAR THE SON OF ABDESASIM THE SON OF HHUR (or HUR)—THE SEPULCHRAL STONE OF LEMB (or LEMEB) WHO LIVED TWENTY YEARS IN TROUBLE AND SORROW — These AMATHUSIANS WHO WERE SLAIN ARE GONE FOR EVER TO THE PRISON of the grave—THE MONUMENT WAS ERECTED BY THE HOUSE OF TAMVS (or TAM) THE SON OF ABDEMELEC.

Hence it seems to appear, that the names of two Amathusians, probably of the first distinction, one of whom was unfortunate enough, have been handed down

down to us, and perhaps to all succeeding ages, by this sepulchral inscription.

19. It must be farther observed, that this curious monument consists of four short periods; every one of which may, in some respect, be taken for a complete inscription. But this is a property it has in common with other similar remains of antiquity. Thus the Sigeian inscription (70) is composed of four such periods, and three are exhibited by the Punic (71) inscription that in a former paper I have attempted to explain.

20. I have hinted above, that the inscription before me is come down to us perfect and incorrupt; not so much as one of its letters having been either lost, or greatly damaged, by the injuries of time. To which I shall now beg leave to add, that the words formed of these letters are, for the most part, distinguished from one another by points, placed between them; which must, in a good measure at least, ascertain the lection here, and of course greatly facilitate the explication. The Etruscans sometimes separated their words from one another by two points, and sometimes by a single one only, as we learn from the Etruscan inscriptions on the celebrated tables of Gubbio, and others published by Sig. Gori, in (72) the learned work referred to, which may be considered as a noble repository of all kinds of Etruscan antiquities. The earlier Greeks also used the first kind of interpunction, as we learn from the

(70) Chish. *Antiquitat. Asiat.* p. 30, 31. Lond. 1728.

(71) *Philos. Transf.* Vol. LIII. p. 279.

(72) Anton. Francisc. Gor. *Mus. Etrusc.* Vol. I. II. pass. Florentiæ, 1737. & Vol. III. pass. Florentiæ, 1743.

Sigean, (73) Teian, and other antient inscriptions. That they likewise applied three points for the separation of their words, on (74) some occasions, tho' more rarely, as well as the Etruscans, is not unknown to those who have been conversant with the antiquities of these nations. I must farther observe, that this minute kind of mark, though generally termed a point, was originally of a triangular form; as may be inferred both from our Citiean inscription, in which some of the minute black triangles plainly appear, and one at least of those preserved by the tables of Gubbio (75), of which so accurate a transcript has been communicated by Sig. Gori to the learned world. That these points are a certain indication of a pretty remote antiquity, is by the most competent judges of such matters (76) readily allowed. How far therefore this interpunction and antient history may conspire, in order to settle the age of the monument under consideration here, I am next to inquire.

Abdemon, the Citiean, one of the Persian (77), monarch's friends, having been expelled Salamine by Euagoras, that prince meditated the reduction of

(73) Chish. *Antiquitat. Asiat.* p. 6, 14, 97, 98. Paul. M. Paciaud. *Monument. Peloponnes.* 207, 209—213, 218, Romæ, 1761.

(74) *Mus. Veronens.* p. 407. Veronæ, 1749. Anton. Francisc. Gor. *Mus. Etrusc.* Vol. III. P. III. T. XVI. Florentiæ, 1743.

(75) Anton. Francisc. Gor. *Mus. Etrusc.* Vol. I. Prolegom. p. 55. Florentiæ, 1737.

(76) Vid. Chish. *Antiquitat. Asiat.* p. 3, 6, 14, 97.

(77) Theopompus in Excerptis Photii, Cod. CLXXVI. *Inscript. Cit.* p. 24—28. Oxon. 1750. Diod. Sic. *Bibl. Hist.* Lib. XVI. p. 447. Isocrat. *Evag.* p. 282.

the whole island of Cyprus; in which, within the course of a few years, he made a very considerable progress. This alarming the Amathusians, Citicans, and Solians, governed then, as it should seem, by their own princes, they (78) made the proper dispositions for opposing his ambitious designs. But not believing themselves able alone to cope with him, they applied to the Persian (79) court for assistance. Artaxerxes Mnemon, who then sat upon the Persian throne, was also himself become jealous of the growing power of Euagoras, and therefore readily entered into an alliance with the three confederated cities against him. To this he was farther excited by the murder of Agyris, king of Amathus, and one of his most faithful allies, of which Euagoras (80) was accused; and by the engagement the three Cyprian states had entered into, to put the whole island, if possible, into his hands. In order therefore to crush Euagoras at once, Artaxerxes sent an army of 300,000 men, under the command of Orontes, one of his sons-in-law, to invade Cyprus (81), in the third year of the ninety-eighth Olympiad, or the year before Christ 386. This formidable army was attended by a fleet of above 300 (82) sail, of which Gaus, the

(78) Diod. Sic. ubi sup. p. 447.

(79) Id. ibid.

(80) Id. ibid.

(81) Diod. Sic. ubi sup. Lib. XV. p. 458.

(82) Id. ibid. The Phœnician name TAM seems to have been written by the Greeks TAMOΞ, as it is exhibited by a MS. of Thucydides, in the French king's library; and not TAMΩΞ, as we find it written in other manuscripts of that author. This is rendered not a little probable, at least in my opinion, by the Oxford-Citian inscription.

Vid. Thucyd. *De Bell. Peloponnes.* Lib. VIII. c. 87. p. 557.
 Ed. Duker. Amstelædami, 1731.

son of Tamus, or, as the Phœnicians wrote and pronounced the word, Tam, probably the TAM of our inscription, was (83) admiral. This Tamus is said to have been born at Memphis, and consequently by birth to have been an Egyptian, though he was probably of Phœnician extraction. Being a person of great valour, and uncommon skill in maritime affairs, he first served Tissaphernes as a naval officer; but was afterwards employed by Cyrus, who rebelled against his brother Artaxerxes, and was killed in the battle (84) of Cunaxa, as chief commander of his fleet. He also had been appointed governor of Ionia by that prince. Tamus was treacherously cut off, with all his family, except his son Gaus, now the Persian admiral, who staid behind in Asia, by Ptammitichus, king of Egypt (85), about fourteen years before. Euagoras's fleet of 200 sail was defeated near Citium (86) by Gaus, the son of Tamus, or Tam, with the loss of most of his ships; though Euagoras had, before this naval engagement, gained a (87) considerable advantage over a part of the combined army of Persians, Amathusians, Citieans, and Solians, almost immediately after the descent had been made. From this short narrative, extracted from writers of the best reputation and authority, are naturally deducible the following observations.

(83) Id. *ibid.*

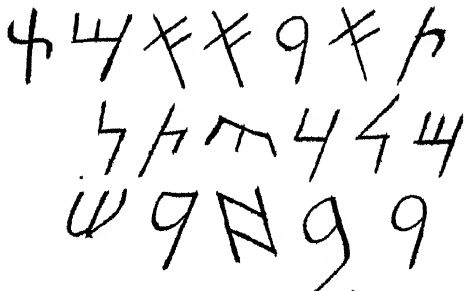
(84) Xenoph. *De Cyr. Expedit.* p. 89. Oxon. 1735. Plutarch. in *Artaxerx.* p. 1014, 1015. Lutetiae Parisiorum, 1624.

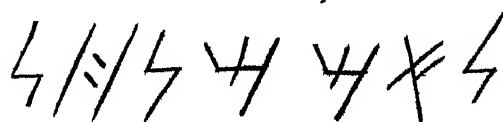
(85) Diod. Sic. *Bibl. Hist.* Lib. XIV. p. 415.

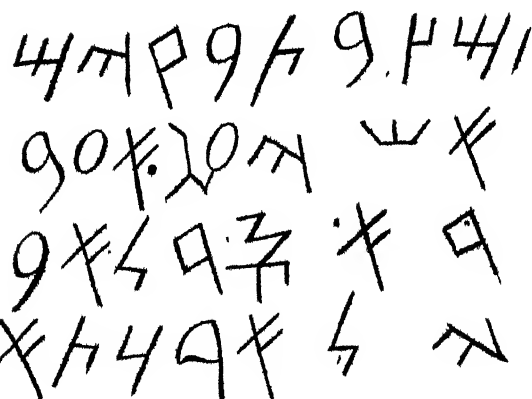
(86) Diod. Sic. *Bibl. Hist.* Lib. XV. p. 459, 460.

(87) Idem *ibid.*

Three of the Cician Inscriptions illustrated here. p. 429, 432, &c.

N^o. 3. 
 Row 1: 4 4 x x 9 x h
 Row 2: 5 h m 4 5 4
 Row 3: w 9 n 9 9

N^o. 21. 
 Row 1: 4 / 5 4 4 x 4

N^o. 23. 
 Row 1: 4 m p 9 h 9 . p 4 /
 Row 2: 9 0 x . 2 0 m w x
 Row 3: 9 x 4 9 3 x 9
 Row 4: x h 4 9 x 5 m

1. Tamus, or Tam, probably the TAM of our inscription, admiral of a Persian fleet, and governor of Ionia, was cut off by Psammitichus, king of Egypt, together with his whole family, except his son Gaus, about fourteen years before the commencement of the Cyprian war.

2. Gaus, the son of Tamus, or Tam, admiral to Cyrus, who was killed on the plains of Cunaxa, actually commanded the Persian fleet, and defeated that of Euagoras, near Citium, in the beginning of that war.

3. Part of the combined army of Persians, Amathusians, Citieans, and Solians, was routed by Euagoras, a little before the naval engagement.

4. From what has been (88) intimated by Diodorus Siculus we may infer, that this action certainly happened at no great distance from Citium; as the battle by sea was fought near that place, and as the fleet and army must have acted in concert, both at the debarkation of the troops, and for some time after that event.

5. It must therefore be allowed probable, that the two Amathusians mentioned in our inscription, who seem to have been persons of distinction, were killed, either in the aforesaid action, in the naval engagement that immediately followed, or in some other affair that happened much about the same time.

6. The monument recorded by our inscription was probably erected by some of Gaus's family, who might call themselves the house of Tamus, his father, several instances of such an appellation occurring

in antient history. This might have happened after Gaus's death, which was about two years posterior to the commencement of the Cyprian expedition. The erection of it certainly ought not to be attributed to Tamus's daughter, as some may perhaps pretend; all that admiral's family, except Gaus, having been cut off with him, (89) by Psammitichus, king of Egypt, fourteen years before.

7. From the preceding narrative we may infer, that antient history, particularly that of Diodorus Siculus, from whence it is chiefly extracted, and our inscription mutually strengthen and support each other.

8. Hence it seems pretty clearly to appear, that the death of Abdasar and Lemb, or Lemeb, the event commemorated by our inscription, preceded the commencement of the Christian æra 386 years; and consequently that this inscription is coeval with those, found likewise in the ruins of Citium, by me some years since explained.

Many things relative to this inscription, for want of room, I am obliged at present to supersede; but these, as well as several other points slightly touched upon here, may perhaps meet with a more particular discussion, in another piece upon the same subject; which, if God grants me life and health, will soon be communicated to the learned world.

As the autograph of the fourth inscription [TAB. XXVI.] does not now exist, having been destroyed by Bekir, bashaw of Cyprus, about the year 1749; we cannot arrive at any degree of certainty, in relation to the

(89) Diod. Sic. *Bibl. Hist.* Lib. XIV. p. 415.

characters of which it was originally composed. Of those, however, exhibited by the transcripts only the last of the first line and the last of the inscription seem to have deviated from their primitive forms. The last character of the first line I can by no means take for *He*, as M. l'Abbé Barthelemy supposes it to be, since it scarce bears a remote resemblance even to his pretended new form of that element. Besides, M. l'Abbé has in effect given up this new form, by allowing that on the coins of Menæ it may be taken for *Mem*. Nor can such an uncouth proper name as תאראאמ, THARAAME, with three *Alephs* in it, and two together in the middle of it, I believe, be found in the whole circle of Hebrew, Syriac, or Phœnician antiquity. I should therefore rather call it *Thau*, one form of which it greatly resembles, if part of the curve behind be considered as an accidental addition; which might easily have been made, in the course of so many ages. With regard to the last letter of the inscription, this seems to be purely adventitious, and of a recent date. For Dr. Porter's accurate copy of this inscription, taken upon the spot, which the late Rev. George Drake, M. A. and Fellow of Balliol College, received of Charles Gray, Esq; Member of Parliament for Colchester, and gave to the University of Oxford, exhibits a character resembling the modern form of *Schin*, though made in a very bungling irregular manner. In short it presents to our view not the least appearance of antiquity. Nay, it seems to have been formed upon the ruins of a *Nun*, the upper part of which is plainly visible in this character. I would therefore, with the permission of the critics, read the

list:

tional light upon part of the preceding remarks, and more clearly evince a point of considerable importance, with regard to the true explication of the Oxford inscription, that has been manifestly opposed by M. l'Abbé.

1. The twenty-first of the Citiean inscriptions, which consists only of the two words לֹא־מָמֶן חָלַל , or לֹא־מָמֶן חָלַל , had probably lost a *Lamed*, before the autograph itself was destroyed; unless we will suppose the Phœnicians of Cyprus, when it first appeared, to have used even the participle *pahul* itself of the radix חָלַל , or חָלַל , in the contracted form. The original Phœnician is equivalent to the Latin *AMAMONO INTERFECTO*, *AMAMONO in acie INTERFECTO*, or *AMAMONI in prælio CONFOSSE*, scil. *lapis sepulchralis, i. e. the grave-stone OF AMAMON KILLED in war*; which seems to imply, that this Amamon, who was probably an officer of some note, fell in the affair that happened between a part of the combined army of Persians and Phœnicians and a body of Euagoras's troops (92) near Citium, soon after the commencement of the Cyprian war. Whence we may infer, that this inscription is coeval with those I have already endeavoured to explain; that it points, clearly enough, at the same event; and consequently that it brings a fresh accession of strength both to my opinion of the age of those monuments, and also to the authority of Diodorus Siculus himself in the point before us.

As for the Phœnician proper name *AMAMON*, I have formerly expatiated so (93) largely upon it,

(92) Diod. Sic. *Bibl. Hist.* Lib. XV. p. 459, 460.

(93) *Inscript. Cit.* p. 20, 21, 22. Oxon. 1750.

that

that nothing farther relative to this word will be expected from me here. The figure of the *Hbeth*, according to Dr. Porter's transcript, not a little resembles the unusual form of the same element as presented three times by the Oxford-Citician marble to our view.

2. The twenty-third Citician inscription is formed of six words, five of which are evidently proper names. They are ranged in four lines, and may be represented by Hebrew characters thus.

רקים	מצבת
עב	אסיעוא
לאר	דאסר
רבתא	ילא

LAPIS SEPVLCHRALIS RECIMI	
ASIAVÆ	AB
DASARI	LAR
ILÆ	RECATÆ

The fifth letter of the second line, which seems to have been intended for *Vau*, is pretty much deformed. This appears from Dr. Porter's accurate copy of the inscription. The original itself does not now exist. I take this character to have represented (94) *Vau*, because it somewhat resembles the Æolic *Digamma*, which answered to that element. The minute strait line preceding the first word is evidently an accidental blemish, and therefore cannot be considered as a letter. This, from its size

(94) Chish. *Antiquitat. Asiat.* p. 17, 19. Lond. 1728.

K k k 2

and

and position, as well as from the term that immediately follows it, is incontestably clear. The two words forming the first line of the inscription are apparently **מצבת רקים**, LAPIS SEPULCHRALIS RE-CIMI, THE TOMB-STONE OF REKIM, or REKEM, the latter of which is (95) a Biblical proper name. The third of the proper names preserved by this inscription, ABDASAR, occurs on the Oxford and Maltese stones, and has been already explained; but the others I remember not elsewhere to have seen, nor are they, as I apprehend, to be met with in any antient author. The persons that bore them were probably killed in the action near Citium, mentioned by Diodorus Siculus, referred to on a similar occasion in these remarks, and buried in one grave. That action, as I have formerly observed, preceded the commencement of the Christian æra about 386 years.

The substantive **מצבת**, in the beginning of a sentence, immediately preceding the proper name of a man, not followed by the term **בן**, BEN, SON, and the father's name, appears in this sepulchral inscription, that has more than one person for its object. This word also occurs, attended by the very same circumstances, in the first line of our Oxford inscription. Now in the Citiean monument before me it is undoubtedly equivalent to the Latin LAPIS SEPULCHRALIS, and the English TOMB-STONE. Why then should it not have the same signification assigned it in the other? Most certainly it should. Can any thing therefore be more forced and unnatural than M. l'Abbé Barthelemy's notion of this term in the

Oxford inscription, when he makes it to refer to I know not what obscure town called Tšabeth, the faintest traces of which are not to be met with in any ancient writer? A notion this void of the least appearance of authority, and destitute of the very shadow of a reason for its support!

The twenty-third Citiean inscription is also remarkable for the exhibition of a very ancient form of *Koph*, and of a pretty unusual one of *Tzade*, resembling the character that represented the same element amongst the Palmyrenes. This likewise sometimes appears upon the Phœnician (96) coins. The figure of *Vau* here seems by some accident to have been deformed, before the autograph was destroyed. It nevertheless bears a sort of rude resemblance to the *Æolic Digamma*, which (97) owed its origin to this letter. As it has deviated, however, considerably from the primitive character, cut at first in the stone; I have not assigned it a place in the Phœnician alphabet, deduced from the inscriptions found amongst the ruins of Citium, and now attending these remarks.

Thus have I finished my remarks upon M. l'Abbé Barthelemy's reflections on certain Phœnician monuments, and the alphabets resulting from them; and endeavoured to rectify some mistakes, that occur in this celebrated performance. How far I have succeeded in my design the learned world, with candour and impartiality, will decide. I have also attempted to explain four of the Citiean inscriptions, in the course of these remarks; and hope the explications

(96) Joan. Baptist. Biancon, *De Antiq. Hebraeor. & Græcor. Lit.* p. 32. Bononiæ, 1748.

(97) Chish. ubi sup.

given, even upon the most critical examination, will not be found very remote from truth. The alphabets deduced from these inscriptions and the Siculo-Punic coins, illustrated here, will, I flatter myself, not a little facilitate the interpretation of other similar remains of antiquity. Nor will M. l'Abbé, for whose superior merit I have a real esteem, find the least reason to complain of any illiberal treatment in this memoir. For as on the one hand, ever averse to flattery, I have delivered my sentiments with a becoming freedom, when I thought myself obliged to differ from him; so on the other, notwithstanding the provocations received from certain authors, I have studiously endeavoured to avoid every thing that might seem to have the least tendency to a diminution of his character, as well as all undue warmth and asperity of expression. Nor am I conscious of having misrepresented him in any one particular. Truth stands in no need of such supports, nay it utterly disclaims them; and truth is considered as the sole object in view here by,

S I R,

Your much obliged,

And most obedient,

Humble servant,

Christ Church, Oxon.
Nov. 24, 1764.

John Swinton.

A N
I N D E X
T O T H E
Fifty-Fourth V O L U M E
O F T H E
Philosophical Transactions.

For the Y E A R 1764.

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T H E E N D O F V O L. L I V.

E R R A T A.

P. 33. l. 2. read thus: $T = \frac{x}{x+1} \Big|^{-1}$

14. read reckoned.

c 18. for $\frac{T^3}{\times 3}$ read $\frac{T^3}{\times^3}$

36. 11. from the bottom, to 8 put to 5.

99. ult. for ~~greatly deformed~~, read somewhat deformed.

ERRATA in VOL. LIII.

P. 240. lin. 15. *from bottom, for M. De la Caille's, read M. De la Caille's Ephemerides.*

241. l. 394. *from bottom, for ecliptic orbs, read elliptic orbits.*

245. l. 9. *from bottom, for lines of conjunction, read times of conjunction.*

177. *for מחננת, read מתנת.*

292. l. 8. *for any relation, read any peculiar relation.*

